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A CRITICAL INVESTIGATION OF THE VISUAL APPREHENSION OF MOVEMENT

By WALTER S. NEFF, Cornell University

The visual apprehension of movement presents a problem which has long had, and which continues to have, a central place in theories of perceiving. Many who have approached the problem have—to be sure—used it merely as a convenient whetstone upon which to sharpen some controversial axe, and, too often, the only outcome has been the sharpened axe, with the problem itself left unresolved. Nevertheless, research has gone on and on and facts have accumulated for a good half-century. The time has now come for an extended survey of the experimental studies upon this aspect of perceiving, in order (1) to summarize those facts about which there has been general agreement, (2) to indicate those items which are still controversial, and (3) to place the problem in its historical and theoretical setting, to the end that future research may proceed upon a firmer and more economical basis.

There are well over a hundred articles in the field. Most of these were written to defend one school against the attack of another, to uphold an old theory, or to propose a new hypothesis. It will be necessary, therefore, to envisage each significant discovery or proposal in its systematic setting, and in doing so we shall find that we have traced the general course of development of the several theories of perceiving.

First we must anticipate certain elementary confusions which run throughout the history of our problem. The most disturbing of these is an insufficient distinction between physical stimuli at a receptor and perceived objects. Another confusion identifies the products of the perceptive functions and the character of the functional operations themselves. We shall find many instances of both confusions in the course of our historical

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survey and we shall have to take account of the many obscurities and false conclusions which proceed from them.

The significant publications in the field we shall take up, as far as is possible, in chronological order, with certain gross divisions into which the studies and theories seem naturally to fall. This arrangement may be violated here and there to permit a closer approximation to historical continuity, as well as to permit convenient grouping by nationality. In the final section of the paper an attempt will be made to gather up loose ends and to indicate the directions which research in this field has already taken and may profitably take in the future.

I. The Forerunners

To Sigmund Exner should go the credit of being the first to discover in the problem of seen movement implications of importance for general psychological theory.[1] Like many other physiologists of his day (his article was published just one year after the first edition of Wundt's *Grundzüge*), Exner was keenly interested in the classification of what were then called 'sense phenomena.' That he was familiar with the writings of Helmholtz we cannot doubt; for we find him citing both the *Tonempfindungen* and the *Physiologische Optik*. It is also significant that his citations refer to the distinction between sensation and perception, for Exner's central problem is just to discover whether the seeing of movement is to be classed under the one heading or the other.

Proceeding from definitions of sensation and perception, which Helmholtz had formulated, Exner makes the claim for a sensation of movement, as men before him had made a like claim for a sensation of space. The evidence for his position he summarizes under four points. (1) The sensation of movement has a limen just as any other sensation has. The movement of the minute-hand of a watch is inferred, while that of the second-hand is directly seen. In the first case, we may talk of a perception of movement, and in the second case of a sensation of movement. (2) The minimal temporal interval for the seeing of movement is less than that which is required for the perceived succession of two lights. He found the interval in the first case to be 14 σ, as against 45 σ recorded for the second. (3) In indirect vision, a movement can still be seen between two places in the field which lie too close together to be resolved under conditions suitable for visual acuity. (4) There is a negative after-image of movement.

From Exner's point of view, the evidence which he marshals in support of his claim is strong. It is further clear that his position derives from Helmholtz's definition of perception in terms of *unbewusstes Schluss* and of sensation in terms

[1] S. Exner, Über das Sehen von Bewegungen und die Theorie des zusammengesetzten Auges, *Sitzber. Königl. Akad. Wiss.* (Wien), Math.-naturw. Kl., Abt. 3, 72, 1875, 156-190.

of the immediately given. Whenever Exner speaks of the perception of movement he refers to it as *erschlossene;* when he wishes to describe the sensation of movement he uses the phrase *Ich sehe.* Throughout his writings he treats all his evidence from the point of view of this distinction.

Now it cannot be denied that Exner's observations are often acute and his illustrations cleverly chosen. On the other hand, it is equally clear that his entire theoretical position stands and falls with the long out-moded Helmholtzian views upon perception. In the light of more modern writings, both experimental and theoretical, all of the phenomena which Exner reports are perceptual in nature; they could be nothing else. His whole discussion turns upon the fact of the irreducible character of the phenomenon and upon his identification of perception with inference. Certainly if we place perceiving outside the sphere of the immediately given, then we are justified in describing seen-movement as non-perceptual. On the other hand, more recent results show that it is exactly this 'immediacy' which is a fundamental characteristic of perceiving. Where Exner speaks of his inferred *(erschlossene)* movement it is probable that we are dealing here with a very complex performance in which inspecting and comprehending play an essential part.

It is clear, then, that Exner's contribution is one-sided. On the side of the event itself, he offers us some interesting observations which have stood the test of experimental control. On the side of performance, however, he presents us nothing but an erroneous and out-moded point of view. We can utilize his phenomenal observations for what they are worth; his theoretical conclusions only possess historical importance.

About twenty years after Exner's work William Stern,[2] who had been studying under Ebbinghaus at Berlin, published a long article on movement as part of his protracted study of the perception of change.

The point of view which Stern brings to his experimental labors is significant, and is decisive for an understanding of his conclusions. For him, all observations of phenomena of the outer world are interpretations *(Deutungen),* made upon the basis of sensory impressions. Again we note the wide-spread influence of the Helmholtzian views on perception, and of the wider doctrine of conscious and unconscious inference. So we find Stern saying "Wie alle Vorgänge der Aussenwelt Deutungen auf Grund sinnlicher Eindrücke sind, so auch die Annahme der Bewegung. . . . Dieser Schluss kann entweder mit Bewusstsein gemacht oder unbewusst vollzogen werden."[3] Here is what may be called an epistomological view of perception; sense-impressions are 'signs,' hints, upon the correct use of which the individual may form a judgment of what is occurring in the external world.

With this point of view adopted, Stern sets himself the general problem as to whether a single sensory impression or a series of impressions is necessary for an 'interpretation' of movement. Starting with a series of impressions, he investigates the perception of movement as a product of several sensational factors. Here

[2] L. W. Stern, Die Wahrnehmung von Bewegungen vermittelst des Auge, *Zsch. f. Psychol.,* 7, 1894, 321-385.
[3] *Ibid.,* 353.

we find a discussion of what Exner had called 'inferred movement.' The idea of a movement can arise if we percieve and compare several phases . . . "und die Überzeugung von der *Identität des Gegenstandes,* vereint mit dem *Bewusstsein vom Anderssein des Ortes* führt zu dem *Schluss* dass das Objekt . . . sich bewegt habe."[4] This is designated as the *Prinzip der Phasenvergleichung,* and is called into play by especially low speeds of movement. Exner's illustration of the movement of the minute-hand of a watch is a case in point.

Before Stern proceeds further, he raises a question which had at the time great theoretical importance. Just as Exner had asked, so we find Stern asking whether there is a specific sensation of movement; and we find, contrary to Exner, that Stern denies the existence of an elementary sensation of movement. It is true that certain sensations or sensation-complexes can be directly interpreted as signs of an outer movement. These sensations, however, form no special group; merely their modes of combination or appearance are important. There is no 'sensation of movement,' as there is of tone or color.

With his position clear upon this point, Stern passes to a discussion of the perception of movement as aroused by a single factor of sensation. Three principles are formulated here—the principles of *veränderten Reizung,* of *Nachbildstreifen,* and of *Willensimpulsen.* Of the first, Stern states that, in general, the sudden appearance of a *new* impression is interpreted in and for itself as referring to an outer movement. The movement, however, may be directionless, since the principle speaks only of changed stimulation.[5] The second principle is based upon the existence of a positive after-image which—assumed to be still present by the time the next stimulation occurs—makes possible the perception of a continuous movement. The third principle is concerned with eye-movements. Stern denies that eye-movements may be the sole condition of the perception, and limits its effect to two possibilities: (1) the perception of the manifold kinesthetic sensations obtained from the jerky pursuit-movements of the eye-ball, and (2) consciousness of the impulses of the will which regulate the movements of the eyes.

It is with these principles that Stern attempts to answer all of the questions raised in a study of the visual perception of movement. The task of the first principle (*veränderten Reizung*) is "to signal movements occurring in the visual field and to direct attention to them." But the further claim is made that any sudden change in stimulation will arouse a perception of movement. This argument appears to rest upon the supposition that anything newly seen will be taken by the observer to have *moved* into his field of vision. On the other hand, sweeping one's eyes over a desk involves innumerable changes of stimulation with little or no perceived movement. The blinking on and off of the electric sign rarely if ever is accompanied by a perception of movement. Changed stimulation may be a condition for this perception only in very special circumstances, and only when accompanied and supported by many other favoring conditions.

Stern regards his second principle (*Nachbildstreifen*) as the essential condition of the perception of movement when the eyes are held stationary. This fixation is the typical case of the bulk of the later experiments in this field. That after-images are not an essential condition of the perception was demonstrated several

[4] *Ibid.,* 353.

[5] Stern reports cases of directionless movement obtained in stimulation of the retinal periphery, *op. cit.,* 359 ff.

years later by Wertheimer,[6] who showed experimentally that movement could be perceived where the stimuli were presented at so great a temporal interval and visual angle that the after-image of the first must have died away before the second stimulation occurred. The principle of *Phasenvergleichung* need not concern us, since it presents an analysis of the much more complex operation of inspected or comprehended movement, which we mentioned in our treatment of Exner. Finally, the last principle of *Willensimpulse* stands or falls with the long extremely dubious 'sensation of innervation.'[7]

What has Stern contributed to our problem? In the first place, he represents a step in advance of Exner in his emphatic rejection of the 'sensation of movement.' Secondly, in his attempt to treat all the known phenomena of movement, he gives some indication of the complexity of the problem. Thirdly, his analysis of the place of identity in the apprehension of slow movements is the first appearance of a problem which later became important. On the other hand, not one of his imposing list of principles has stood the test of experimental verification, and his actual experiments were too limited to be called in any sense decisive. His work represents an historical advance but is too clearly possessed of historical limitations.

Twenty-three years after the appearance of Exner's paper and four years after Stern's, Karl Marbe, a student of Wundt and Külpe, published a work which drew heavily upon Wundt's systematic position for its general approach and outcome.

For Marbe the discussion of the probem falls into two parts.[8] The first deals with what occurs at the peripheral mechanism, in the ocular system itself. In the second we find a treatment of the contributions of the central structures. The whole tone of the article corresponds to that of the larger proportion of physiologico-psychological studies at that time issuing from the Leipzig and Göttingen laboratories. Like the investigations of Geiger, Meumann, Kirschmann and others, this study of Marbe's bears very prominently the mark of Wundt's theories of integration.

As for the specific problem of seen movement Marbe reduces the phenomenon to the fusion (*Verschmelzung*) of successive periodic retinal excitations, and therewith to Talbot's laws of fusion. He utilizes the concept of *unbemerkte Phasenausfall* and attributes the unnoticed 'falling out' of the phases of tachistoscopically presented movement to fluctuations and errors of attention. As evidence in support of the fusion hypothesis, he offers the observation that there is a certain minimal rate of succession of discrete stimuli below which movement will not be

[6] M. Wertheimer, Experimentelle Studien über das Sehen von Bewegung, *Zsch. f. Psychol.*, 61, 1912, 181 ff.

[7] For a treatment of the 'sensation of innervation,' see Müller and Schumann, Über die psychologischen Grundlagen für die Vergleichung der gehobenen Gewichte, *Arch. f. d. ges. Physiol.*, 45, 1889, 80 ff.

[8] K. Marbe, Die stroboskopischen Erscheinungen, *Phil. Stud.*, 14, 1898, 376-401.

seen. Both of these results are interpreted in terms of Talbot's laws.[9] He develops his view of *unbemerkte Phasenausfall* as follows: if we present movement stroboscopically . . . "die Ausfall von Bewegungsphasen in Folge centraler Ursachen unbemerkt bleibt . . . bei ungenügender Aufmerksamkeit und ungeeigneter Richtung derselben."[10]

Marbe thus bases his discussion of the perception of movement upon experimental evidence from other fields—from the theory of retinal excitations, and from current views of attention. Let us see if these applications are justifiable. First of all, the case for fusion. It is clear that Marbe confuses the stimuli presented with the object perceived. He would have us believe that the appearance of a continuum arising with discretely presented stimuli is dependent upon a sensory fusion of the images upon the retina. Now, to identify the appearance of a continuum with fusion is to give the term fusion an entirely novel meaning. In typical cases, as with rotating disks, we speak of fusion only when the colors, perceived as distinct when the disk is stationary, blend into an homogeneous surface upon rotation. The matter is quite different with stroboscopically presented objects. Under stroboscopic conditions, the report is in terms of an object appearing at one place in the field, moving over to another place, and disappearing in this final position. We find no observation which is couched in terms of an homogeneous light surface. Marbe has been misled by an incidental similarity in two totally different experimental situations.

The case for fluctuation of attention as a determiner of the perception of movement is even worse. Marbe reports that the perception of movement appears to break down if the attention is kept fixed directly upon the stimulus-objects. Therefore, he attributes the unnoticed dropping out of phases to errors or fluctuations of attention. Here we appear to have experimental evidence. On the other hand, the evidence presented by Marbe is directly controverted by the careful experiments of other men. Among others, both Linke and Wertheimer report full movements obtained with the most careful direction of the attention and maintenance of fixation upon the *Zwischenfeld*.[11]

Marbe's attempt to force the problem of movement within the confines of Wundt's systematic framework turns out to be an utter failure. In addition to the theoretical arguments against his application of Talbot's Law, there is the quantitative evidence of many men upon the temporal interval.[12] His investigation of attention as a factor is even more dubious. In general, his work in this field appears to have been careless. His controls are inadequate or altogether lacking: his understanding of the implications of his material is limited: his conclusions are hasty and dogmatic. But he does make one important contribution. The perception of movement becomes, at least in part, a central phenomenon, and, from his time on, with one exception (Dürr), it is invariably discussed in terms of the cen-

[9] *Ibid.*, 386 ff. [10] *Ibid.*, 399.

[11] P. Linke, Die stroboskopische Täuschungen und das Problem des Sehens von Bewegungen, *Psychol. Stud.*, 3, 1907, 479 ff., and M. Wertheimer, *op. cit.*, 204 ff.

[12] See our summary below. The range of intervals is too wide to allow any hypothesis of retinal fusion.

tral resources of the organism. Seen movement is no longer a 'sensation.'

The investigation of Ernst Dürr[13] falls into the same systematic framework as that of Marbe and, proceeding from the same assumptions, appears as an attempt to correct certain of Marbe's conclusions.

Accepting his predecessor's attack upon the problem in terms of fusion and *unbemerkte Phasenausfall,* Dürr rejects that part of Marbe's explanation which refers to central phenomena (attention) and tries to base his own theory entirely upon the properties of the peripheral mechanism itself. In so doing, his study represents the last attempt to explain the phenomena of movement in entirely peripheral terms.

Since Dürr's doctrine of fusion and his applications of Talbot's Law take essentially the same form as in Marbe, there is no need for a renewed discussion of these points. Dürr's main difference from Marbe lies in his answer to the question Why does the dropping out of phases in stroboscopic movement remain unnoticed under certain conditions, and what are these conditions? Marbe's answer had been in terms of the direction of the attention. Dürr instructs several observers to direct their attention now on the place of the dropped-out phases, now away from that place, and he concludes that, although the direction of the attention has some effect upon the event, its effect is not decisive.

In the course of these experiments Dürr discovered what he thought to be an important fact, which he formulated as a leading question. "Warum bei Fixation eines festen Punktes ein Phasenausfall ohne weiteres bemerkt wird, während er unbemerkt bleibt, wenn eine Fixation nicht stattfindet."[14] For Dürr, then, the situation is clarified at once. Where some phases of the movement are lacking, and the glance is fixated upon the gap, the movement appears to break down. Where, under the same sitmulus-conditions, the glance is allowed to follow the successively appearing stimuli, good movement is perceived. What have we here, Dürr exclaims, but a dependence of the phenomenon upon shifts in fixation, upon eye-movements? Here Dürr rests his case. Movement is entirely explained in peripheral terms.

Dürr's contentions do not necessitate an extended discussion for their refutation. If the criticisms directed against Marbe's hypothesis of fusion are justified, then they are equally in place here. Like Marbe's claim for attention, that of Dürr for eye-movements, as the second essential condition for the perception of movement, has gone the way of experimental disproof. An impressive list of investigators (Linke, Wertheimer, Korte, Kenkel, Hillebrand, DeSilva and others) have consistently obtained full movement with strict fixation upon the *Zwischenfeld.* Dürr's error may be traced to limitations of apparatus. Examination of his tables discloses the fact that his exposure-times were too brief and his temporal intervals

[13] E. Dürr, Über die stroboskopischen Erscheinungen, *Phil. Stud.,* 15, 1900, 501-523.

[14] E. Dürr, *op. cit.,* 515.

too long to present the best conditions for the arousal of a perception of movement under fixation. With the refutation of Dürr's claim for eye-movements, his article is at once stripped of importance.

In 1918, eleven years after his first experimental study of our problem and six years after the publication of Wertheimer's work, Paul Linke included in a monograph upon perception an exhaustive treatment of the problems of movement.[15]

Linke states that the chief source of error in the work done in this field was an inadequate descriptive determination of the facts. A connection had been sought between the real and the intentional (or phenomenal) while it seems more certain to Linke that the problem really begins earlier, that it lies in the intentional or phenomenal data themselves. For Linke, only the perception itself is the subject of investigation, not those physical events which may lie at the basis of it in reality. The problem is not at all how a real, stationary object is seen as moving, but how a single perceived object, taken as stationary, in combination with another object of which the same holds good, appears as moving.

From this statement of the problem Linke proceeds to a descriptive analysis. He begins by pointing out that although we have spoken of two objects, yet, in a single observation, we never see two objects. What is actually seen is a single object, which proceeds from a given primary position *a* to a given final position *b*. It is true that two objects appear if the temporal interval is lengthened sufficiently. For Linke, however, this simply means that, under optimal conditions, the two objects have been 'identified.' He hastens to add . . . "das soll selbstverständlich nicht heissen, die Stricke würden zunächst immer unterschieden . . . um dann bewusst identifiziert zu werden, sondern es besagt nur . . . dass im jeweiligen Versuch stets nur ein einziger Strich gesehen wird.[16] We meet here Linke's notion of identity, upon which he places great importance.

It must be observed, further, that, under certain conditions, no moving object is seen between the places *a* and *b*. The perception of movement is maintained whether I notice, or do not notice, the absence of a visible path of movement. According to Linke, this apparent paradox can be resolved if we introduce the doctrine of assimilative perception. By assimilative perception he means . . . "die Teilnahme von etwas an und für sich nicht eigentlich wahrnehmungsmässig Gegebenen an dem unmittelbaren Wirklichtkeitseindruck eines eigentlich Wahrgenommenen."[17] The whole problem of the perception of movement, then, falls into two sharply contrasted divisions. On the one hand, there is that which is actually perceived—the initial and final positions of an object; on the other, there is that which is perceived in assimilative supplementation—the moving object in the *Zwischenfeld*.

Linke makes it clear that the most important single determiner of the perception is his conception of identity. He further emphasizes the fact that if there are differences in wave-length, form, or position between the first stimulus-object and the second, nevertheless it is a certain 'something' (*Etwas*) which changes in color, form, or position during the course of its movement. We perceive an object chang-

[15] P. Linke, *Grundfragen der Wahrnehmungslehre*, 1918, 269-360.
[16] *Ibid.*, 278.
[17] *Ibid.*, 280, Anm. 1.

ing certain of its qualifications. If the first stimulus-pattern is one line and the second two lines, then the stroboscopic movement depends upon seeing the final presentation as a doubling of the first. Linke emphasizes the illustration by the simple, yet for him, very important statement that 'twoness is different from doubleness.'

Before we estimate Linke's contribution to our problem, let us glance for a moment at his psychological antecedents. First of all, it must be remembered that he was a student of Wundt, receiving his doctoral degree at Leipzig in 1901. Very early, however, he came under the influence of Meinong and Husserl, so that we find a gradually increasing preoccupation with *Phänomenologie* and *Gegenstandstheorie*. Here, as in the case of Külpe, we have the difficult task of placing a man who was passing over by imperceptible degrees from the psychology of Wundt to that of Brentano. His early work, of which his first articles on movement serve as an example, is strongly tinctured by Wundt's systematic views. Yet, eleven years later, we find him publishing his *Grundfragen der Wahrnehmungslehre*, a serious attempt to treat the results of experimental psychology from the point of view expressed by the writings of Brentano, Meinong, and Husserl. That the influence of Wundt was still effective to some degree is evinced by his treatment of perception in the *Grundfragen* and his analysis of movement in terms of assimilation.

Linke's most important single contribution is his emphasis upon the part that identity plays in the perception. In effect, he says that the individual will perceive movement only if the two phases seen are identified as referring to one object, and that this is the fundamental condition, the condition *sine qua non,* of the perception. This is Linke's main thesis in 1907, and it is urged with even greater warmth in the *Grundfragen,* eleven years later. Unfortunately, his treatment is affected by a fundamental confusion, to which we have referred in our discussion of other men. He speaks throughout of an *Identifikation* of *two* objects, seeming to confuse his two stimulus-energies with the perceived object. Since the experiments of Kenkel and Benussi (cf. below), we know that movement may be perceived where we are concerned with the brief presentation of only one stimulus-object. There is no reason, even in the case of *gamma* movement, why we cannot refer the different phases of the movement to one-and-the-same object. As a matter of fact, the reference seems to be more forced in this expansion-contraction movement than in the other varieties. If Linke discarded his *Identifikation* in favor of a reference of all changes to one-and-the-same object, he would have had a much stronger case. At the same time, his discussion raises an interesting question. Even if we put aside all logical considerations, and confine ourselves to the psychological factors alone, there still remains the possibility that a predisposition to refer all change to one object may be an important condition of the apprehension of movement. If we re-orientate Linke's conception, we at once raise the important question of the effect of 'tuning,' 'predisposition,'

and 'attitude' upon perceiving. Linke's analysis of the perception in terms of assimilation is of far less significance for us. The treatment presents a recasting of older sensationalistic views and will be discussed with those it most closely resembles in the final section of this paper.

II. Wertheimer and the Configurationists

To treat Wertheimer's work on movement[18] with respect to all its implications for modern *Gestalttheorie* would be a task far beyond the prescribed scope of this paper. What we shall attempt here is to value the specific contributions which Wertheimer made to the problem of movement, and to discuss briefly those papers of his followers which are significant.

Wertheimer's fundamental contribution lies in his treatment of the perception of movement in terms of a simple, unanalyzable experience. The opinion of the great majority of psychologists was that perceiving was a complex operation, depending upon an integration of sensory elements and higher central processes. Wertheimer, on the other hand, speaks of a perception *per se*, a direct apprehension of movement, which cannot be analyzed down to any experiential elements. He observes that movement may be perceived where there is nothing of line, color, object, or anything else to be described in terms of 'mental content.' This objectless movement, *Bewegung ohne bewegtes*, is the much discussed 'pure' *phi-phenomenon*. It is with *phi* in mind that he argues against Linke's theories of identity, since we cannot speak of an identical object where there is nothing of object to be seen. Finally, it is from the descriptive analysis of *phi* that he develops his physiological theory of *Kurzschluss*.

Wertheimer approaches the whole problem from a descriptive point of view. His method of experimentation set the pattern for most of the later work in this field. It consisted simply of the presentation of two stationary stimuli, allowed to appear simultaneously, over-lapping in time, or in succession. The possible variables in this situation comprise an immense number. Some of the more important variables are the exposure-time of the stimuli, the temporal interval, characteristics of the stimuli (form, size, conventionality, 'meaningfulness,' color, 'insistence,' intensity), as well as many possibilities upon the side of observational attitude, instruction, and so on. Much of the later work was concerned with investigation of the effect of one or more of these variables upon the perception.

Wertheimer's own discussion places paramount importance upon the temporal interval. It is in connection with this variable that he develops his three *Hauptstadien* (simultaneity, optimal movement, and succession), as well as the various sub-phenomena to be distinguished between them, and it is upon the temporal interval that his physiological theory is based. So we find his *Os* reporting simultaneity at intervals of about 30 σ, optimal movement ranging about 60 σ and succession at about 200 σ. Proceeding from the time of optimal movement to that of simultaneity, various sub-phenomena occur. Using intervals of approxi-

[18] M. Wertheimer, Experimentelle Studien über das Sehen von Bewegung, *Zsch. f. Psychol.*, 61, 1912, 161-265.

mately 45 σ, and over 60 σ, bimembral movements (*duale Teilbewegungen*) are observed, where object *a* appears, makes a small movement, disappears, object *b* appears from the direction of *a*'s final position, makes a small movement and disappears. At intervals close to 30 σ, intramembral movement (*Innenbewegung*) takes place; this is described as an independent flickering and oscillating of the two objects. Scattered between the two extremes of simultaneity and succession, unimembral movements (*Singularbewegungen*) are seen, where only one of two visible objects undergoes movement.

At temporal intervals just adjacent to the 'optimal' intervals, a new and, for Wertheimer and his followers, a very important phenomenon develops. Pure movement, movement exhibiting nothing of *Zwischenlagen,* movement without reference to an object, is reported by both of his *O*s. Here we have the appearance of 'pure *phi,*' about the existence, characteristics and importance of which much later discussion has centered. In addition to the observations reported, Wertheimer carried on decisive experiments upon the rôle of eye-movements in the perception, investigated the effects of change in attention and attitude, verified the presence of an after-image of apparent movement, and instituted a phenomenal comparison of real and apparent movement.

What may be said of Wertheimer's contributions? First of all, his analysis of movement into three states plus various sub-phenomena has affected the whole character of later investigation. This analysis suggests a very interesting question which has been wholly overlooked during a score of years of research and discussion. At basis, Wertheimer appears to have confused two issues: (1) what phenomena attend the successive presentation of two stationary stimuli, and (2) what are the conditions of the perception of movement? At first glance, one might suppose that these two issues are really one, that one is simply a re-phrasing of the other; but it should be emphasized, once and for all, that this is a mistake and a confusion, a confusion to which most of the workers in the field have been subject. It is not to be denied that, under certain very special conditions (conditions about which there has been some lack of agreement), presentation of two successive, in-themselves-stationary stimuli may arouse a perception of movement. But it is not to be derived from this fact that all the phenomena attendant upon the successive exposure of two stationary stimuli are of significance for the problem of the perception of movement. Wertheimer tells us that, under certain conditions, *O* will not report full movement of one object, but that he will report the appearance of two objects, each of which has undergone a movement (*duale Teilbewegungen*). Is this to be interpreted as a stage, as a mode of appearance of movement, or is it simply to be ascribed to limitations of apparatus and organism? Examination of the alternatives and of the experimental literature inclines one strongly to the latter hypothesis. As far as we are

concerned, under Wertheimer's optimal conditions we have *one* moving object in the field and, under conditions for *duale Teilbewegung* we have *two* moving objects in the field. In these cases, we have not different types or stages of movement, as Wertheimer implies, but only *different objects moving*. This statement becomes important when we remember Wertheimer's argument that only in optimal movement may we speak of identity, that the other forms are movements *ohne Identität*. Wertheimer is guilty of the same error which Linke made when he spoke of an *Identifikation* of two objects; and his error arises from the same source—from a confusion of what is actually presented with what is perceived.

The second of Wertheimer's major contributions was his discovery of the *phi-phenomenon* and the implications which he drew from it. Here we have the first appearance in the literature of objectless movement, of movement unrelated to anything in the field. It is to *phi* that he turns, as a last resort, to refute all his predecessors who spoke of supplementation or of *Zwischenlagen,* and it is upon *phi* that he bases his physiological theory. In the light of the work of his followers, this 'pure movement' and the theory devised to explain it became of paramount importance. That it can be observed is attested by many men. Wherein lies its importance? For Wertheimer it was glorified because here we seemed to have an experience of movement *sui generis,* which could be analyzed no further. There was nothing in it to be described in terms of quality, intensity, extent, or any of the traditional terms of introspective analysis. Furthermore, it seemed not to permit of analysis into that which is perceived and that which is supplemented—as was attempted by Linke before Wertheimer, by Benussi and Schumann contemporaneously, and by many others since.

Let us examine this *phi* a little more closely. First of all, phenomena comparable to *phi* have been demonstrated in other fields of perception. Katz's film and bulky color, sounds which are so delivered that they lack localization, and ambiguous pressures; all these, and many other phenomena, have at least one thing in common—they are usually reported as objectless, as having no reference to any object at all. If one were to write a phenomenology of movement, as Katz has done for color and pressure, it would not be surprising if objectless movement was found to be one of the 'modes of appearance' of movement. At the same time, to suppose that objectless movement is the fundamental phenomenon of movement, from which all other phenomena of moving objects are to be explained, is to make a gratuitous assumption. 'Pure *phi*' is only an abstractive form of the general apprehension of moving objects and not at all deserving of

the fuss once made over it. Rather than the 'mental stuff' out of which all perceptions of movement are built, it represents nothing but a bit of interesting observation under fairly specialized conditions. Incidentally, this reading of *phi* deflates it for *gestalttheori.*

What can we say of the third of Wertheimer's major contributions, his physiological theory? This doctrine, elaborated and generalized by his followers, has become very important for *Gestalttheorie.* It attempts to bridge the gap between the two stimuli by assuming a directed dynamic process between two excited places in the cerebral cortex, a sort of cortical short-circuit between two regions of high potential. In Wertheimer's theory we see the prototype of the physiological theories made so much of by the configurationists, and, in the last analysis, discussion of it would be unprofitable. It merely represents another more or less plausible guess as to what goes on in the central nervous system as the physiological correlate of psychological function. Attempted criticism of it would be pointless and an arbitrary denial of its truth would be dogmatic. More recent observations upon temporal interval and seen movement have considerably lessened its plausibility.

In general, Wertheimer's long investigation was so stimulating, it suggested so many problems which it did not even attempt to solve, it was so keenly controversial and contained so many apparently novel assumptions that, during succeeding years, the psychological journals were deluged with ambitious studies of the perception of movement. From now on, the field loses its apparent character of disinterested research and becomes a battleground of systems and points of view.

The first repercussions of Wertheimer's study occurred, as is to be expected, from the members of his newly formed, psychological 'school.' A year after the appearance of his article, Koffka began editing his *Beiträge,*[19] in which many of the contributions of the *Gestalt* experimenters have appeared. Deriving his initial impetus from Wertheimer's study of movement, and particularly from his predecessor's physiological theories, Koffka sets his students the task "to investigate the relationships of dependence between stimuli and *Gestalten.*"

The first investigation appearing under his guidance is that of Kenkel,[20] who was to duplicate Wertheimer's experiments, using figures differing in apparent size (Müller-Lyer patterns, etc.) instead of figures differing in objective size or position.

[19] K. Koffka, Beiträge zur Psychologie der Gestalt- und Bewegungserlebnisse: Einleitung, *Zsch. f. Psychol.,* 67, 1913, 353-358.

[20] F. Kenkel, Untersuchungen über den Zusammenhang zwischen Erscheinungsgrösse und Erscheinungsbewegung bei einigen sogenannten optischen Täuschungen (Beiträge, u.s.w., I), *idem,* 358-449.

Using the methodology and apparatus developed by Wertheimer, Kenkel investigated many variants of figures displaying typical differences in apparent or perceived size. In the course of his investigations he discovers three "funktionell verschiedene Bewegungstäuschungen: (1) die α-Bewegung, die durch zwei örtlich verschiedene Sukzessivreize hervorgerufen wird, (2) die β-Bewegung, die durch zwei einander deckende, aber verschiedene Erscheinungsgrössen besitzende, sukzessiv dargebotene Reize entsteht, (3) die γ-Bewegung, die mit dem Erscheinen nur eines Objektes verbindet."[21] All of the results of Wertheimer (three 'states' and dependence upon interval, uni-, bi- and intramembral movements, effects of attitude, attention and fixation) are confirmed for these new 'forms of movement,' a series of results which does not surprise, since they were obtained with identical methods and apparatus. The discovery of γ-movement, expansion-contraction movements observed under presentation of a single stimulus, later became very important for the *Gestalttheorie,* serving as the basis of Köhler's development of Wertheimer's physiological theories. Kenkel ends his study with a polemic against Benussi, chief experimenter for the *Vorstellungsproduktion* theorists of the Graz 'school,' who had carried on somewhat similar experiments in 1912. No attempt is made to cast these results into the mold of Wertheimer's theory of *Kurzschluss.*

In the second of the *Beiträge,*[22] Koffka makes the ambitious attempt to set up generalizations upon the relations found between the experience of movement and the characteristics of the stimulus, an attempt which resulted in the statement of what have become known as 'Korte's laws.' According to Wertheimer, the direction of the movement perceived depends upon the priority of the *Umkreiswirkung* of the stimulus exposed first. Koffka now sets a student, Korte, the task of discovering whether this priority can be overcome by a greater 'insistence' (*Eindringlichkeit*) of the second stimulus, an insistence augmented by increased physical intensity, direction of the attention, fixation, or other factors.

Under such conditions, Korte reports success in obtaining what he calls *Gegenbewegung* (also δ-movement), where the movement is in a direction opposite to the order of appearance of the stimuli. In the course of his investigation, Korte makes a number of generalizations about the conditions of arousal of δ-movement. He then extends these generalizations to the more common case of β-movement, and formulates his conclusions in a series of laws. These laws attempt to express relationships between the perception of movement and the following variables; the absolute intensity of the stimuli, the distance between them, their exposure-time, and the temporal interval between their successive appearance.[23] He then proceeds to interpret his laws in terms of dynamic events in the central topographical field, in the style of Wertheimer.

If Korte's conclusions had stood the test of later experimental control, they would have comprised an invaluable contribution to the psychology of the apprehension of movement. Unfortunately, his generalizations were

[21] *Ibid.,* 442.

[22] A Korte, Kinematoskopische Untersuchungen (Beiträge, u. s. w., II), *Zsch. f. Psychol.,* 72, 1915, 193-296.

[23] *Ibid.,* 277 ff. For a later discussion of these 'laws' see K. Koffka, Die Wahrnehmung von Bewegung, in Bethe's *Handbuch der normalen und pathologischen Physiologie,* XII, 2, 1931, 1166-1214.

affected by the limitations of his technique. Instead of general laws, he seems to have set up nothing more than a series of rules for the effective use of his own apparatus and methodology. Of later investigators, Neuhaus in particular, using an apparatus which permitted much greater variability in exposure-time, distance and interval, showed that these laws have only a limited effectiveness and, in many cases, no effectiveness at all.[24]

The next few years were spent by Koffka in propagandizing this new point of view and attempting to give it a firm theoretical basis. A few months after Korte's study appeared, Koffka published a long polemic[25] against Benussi, in which he showed the fallacies of the latter's concept of *Vorstellungsproduktion,* and developed his own position. Four years later there appeared an ambitious attempt by Koffka[26] to give Korte's laws a foundation in mathematical theory.

In 1921, Koffka again turns to experiment. With a student, Cermak,[27] he resurrects an old idea (see Marbe, Dürr) and attempts to construct relationships between the phenomena of movement and those of retinal fusion. Accepting Korte's laws as decisive for all phenomena of movement, the question is asked whether these laws also hold when the distance between the phases is reduced, and when more than two phases are presented. A series of experiments is established in which comparisons, both phenomenal and objective, are made between movement (Wertheimer's three 'states'), flicker, and fusion. By way of conclusions, Koffka suggests eight parallel laws, in the style of Jaensch, which are to hold for both movement and fusion.

Close scrutiny of these 'parallel laws' of Koffka's discloses the interesting fact that the relationships which they summarize are relationships between the *Simultanstadium* and fusion on the one hand, and between the *Sukzessivstadium* and flicker on the other. It is found that certain circumstances which favor the perceived simultaneity of two successive stimuli also favor a perceived fusion under conditions of intermittent stimulation, and that the same holds good for perceived succession and flicker. What, may we ask, has this to do with a proposed relationship between movement and the facts of fusion. As we have pointed out before, Wertheimer's *Stadien* should not be regarded as types, or even stages, of movement. They are simply some of the phenomena, other than movement, which attend the successive presentation of visual stimuli. Koffka repeats the error originally made by Wertheimer (cf. p. 11) and displays an even greater confusion. The parallel laws express relationships between fusion and certain tachistoscopic phenomena; unfortunately, they express no relationships between fusion and movement.

During this period since Kenkel's work, δ-movement had been assuming in-

[24] W. Neuhaus, Experimentelle Untersuchung der Scheinbewegung, *Arch. f. d. ges. Psychol.*, 75, 1930, 315-458. See also our discussions of Higginson, Waals and Roelofs, and McConnell, below.

[25] K. Koffka, Zur Grundlegung der Wahrnehmungsypsychologie (Beiträge, u. s. w., III), *Zsch. f. Psychol.*, 73, 1915, 11-90.

[26] K. Koffka, Zur Theorie einfachster gesehener Bewegungen (Beiträge, u. s. w., IV), *Zsch. f. Psychol.*, 82, 1919, 257-292.

[27] Cermak and Koffka, Untersuchungen über Bewegungs- und Verschmelzungsphänomene (Beiträge, u. s. w., V), *Psychol. Forsch.*, 1, 1921, 66-129.

creasing importance for the fundamentals of *Gestalttheorie.* The task is set to Lindemann,[28] then, to investigate all the experiences of movement arising from the presentation of only one stimulus. The conclusion at which he arrives is that δ-movement gives phenomenal expression to the specific dynamic process assumed to be at the basis of the rise of any *Gestalt.* So, if we present outline figures of ellipses, circles or triangles, which lack small pieces of their contours, we find that the lines tend to close their gaps during δ-movement. Furthermore, no δ-movement can be observed in a figure which does not permit the rise of a *Gestalt, i.e.* a haphazard collection of points; as soon as the points constitute a *Gestalt,* δ-movement makes its appearance. Although we noted that figures with small gaps tend to be closed in δ-movement, figures with large gaps in their borders tend to break up into many figures, each of which carries on a movement independently of the others. As Lindemann sums up the *gestaltlich* character of δ-movement, "alle auftretenden Bewegungserscheinungen sind entscheidend bestimmt von der Tendenz zur guten und einfachen Gestalt."[29]

The next important theoretical step was taken by Köhler, continuing his interesting speculations[30] upon the electrodynamics of the brain-field. Köhler had recognized that Wertheimer's famous *Kurzschluss* theory had only accounted for 'pure *phi,*' that it had not been extended and elaborated to cover the much more common case of 'optimal' movement. *Phi* is the rare objectless movement, but, under Wertheimer's optimal conditions, we see an object, complete in form and color, move through the field; this is the typical movement phenomenon of most of the workers in the field. Köhler therefore undertakes a modification[31] of the Wertheimer theory which would account for both *phi* and optimal movement.

From the work of Lindemann we had learned that, if stimulus *a* is given alone, a disturbance arises in the optical sector of the brain-field which forms the *Gestalt A.* The same holds good for stimulus *b.* How can two processes become one, and how does this result in a perception of movement? To answer this question Köhler makes an important statement, which may be taken as the final word of *Gestalttheorie* upon this topic: "Sobald Reiz *a* einwirkt, steigt jedenfalls von der betrefenden retinalen Fläche eine Art 'Säule' von Stromfäden in höhere Gebiete des optischen Sektors auf. Ehe diese Säule wieder verschwunden ist, beginnt eine zweite gleicher Art von der Reizfläche *b* . . . unter diesen Umständen die Kräftegruppierung im ganzen System die beiden Stromsäulen schon unterhalb des psychophysischen Niveaus in Richtung aufeinanderzu ablenkt, so dass sie sich vereinigen."[32] From this statement certain possibilities follow: (1) if the first column is fully built up and the second is just arising, the union is accomplished by a shifting of the second process; as *a* dies down, the whole union of *a* and *b* is shifted toward the now superior position of *b;* (2) if the shifting is not too rapid, chemical transformations occur which supply the color of a moving object (optimal

[28] E. Lindemann, Experimentelle Untersuchungen über das Entstehen und Vergehen von Gestalten (Beiträge, u. s. w., VII), *Psychol. Forsch.,* 2, 1922, 5-60.
[29] *Ibid.,* 58.
[30] W. Köhler, *Die physischen Gestalten in Ruhe und im stationären Zustand,* 1921, 1-263.
[31] W. Köhler, Zur Theorie der stroboskopischen Bewegung, *Psychol. Forsch.,* 3, 1923, 397-406.
[32] *Ibid.,* 399.

movement); (3) if the two columns do not unite, transpositions and shiftings still occur in this highly unstable system, and the bi-, uni-, and intramembral movement of Wertheimer are seen. Köhler attempts to fill the gap in Wertheimer's theory by supposing a chemical transformation-process to accompany the dynamic process of Wertheimer, thus casting the whole in the terminology of modern electrodynamics.

The last important study of movement to appear under the aegis of *Gestalt* was that of Annie Stern,[33] who investigated the perception of movement when the successive stimuli are presented in the region of the blindspot. If stimulus *a* falls on one side while stimulus *b* falls on the other side of the optic disk, what phenomena arise? In brief, Stern's *Os* very often report movement under these conditions, a convincing refutation of purely retinal theories of movement.

The final words upon the perception of movement from the point of view of the *Gestaltpsychologie* have been written by Koffka, who contributed a long and heavily documented article upon this topic to Bethe's *Handbuch*.[34] In this paper Koffka summarizes all the work upon both real and apparent movement and consistently interprets the results in terms of the theory which he has championed for so many years. No positive contributions are made, however, which are not contained in earlier studies by this school, and the article may be passed by with the remark that it constitutes an invaluable work of reference in this very extensive field.[35]

III. Other German Investigators (1912-1933)

The experimental studies on movement by the writers discussed so far have had a profound effect upon the problem. The long controversy has centered about the points raised and about the theories proposed to account for the discovered phenomena. Exner, Stern, Marbe, Dürr, Linke, Wertheimer and his school, may be considered as representative of the influences which have shaped our problem into the form which it possesses today. Beside these men have stood a number of others who have tried, more or less ambitiously, to put an end to the discussion by a conclusive experiment or two, but who have often succeeded better in the obfuscation than in the clarification of the main issues. This historical and critical survey will deal more briefly with these men than it has with the researchers so far studied.

Friedrich Schumann, principally through the work of his students, has for many years occupied himself with this problem.

Schumann's first published words on it may be found in a discussion of a short paper by Linke.[36] Five years later appeared the first of a series of studies carried

[33] A. Stern, Die Wahrnehmung von Bewegungen in der Gegend des blinden Flecks (Beiträge, u. s. w., XI), *ibid.*, 7, 1926, 1-15.

[34] K. Koffka, Die Wahrnehmung von Bewegung, Bethe's *Handbuch der normalen und pathologischen Physiologie*, XII, 2, 1931, 1166-1214.

[35] Our estimate of the contributions of the configurationists to the problem of the perception of movement may be found in the final section of this study.

[36] F. Schumann, *Ber. u. d. 2. Kongr. f. exper. Psychol.*, 1907, 218.

on under his direction and published under his editorship.[37] After this article, which was chiefly critical in character, there was an interruption of fifteen years. The series was resumed with a study of the after-image of movement,[38] continued by an analysis of Kenkel's δ-movement,[39] supplemented by an investigation of what may be seen in the field between the initial and the final positions of movement,[40] and completed to date by a paper upon the effects of changes in brightness and figural character upon the perception of movement.[41]

It is only in Lasersohn's article that we find anything even remotely resembling a general statement of Schumann's views upon the problem. The other studies are wholly experimental in nature. Denying Exner's claim for a sensation of movement *sui generis,* Lasersohn says that in seen movement we deal with a direct apprehension. Nowhere does he attempt any explanation of what the term 'direct apprehension' (*unmittelbare Auffassung*) means. At least, we can assume that this term constitutes a denial of any conception of perceiving which speaks in terms of 'assimilation' (Linke) or 'interpretation' (Stern). We also know that Schumann holds no brief for a 'sensation' of movement. If we turn for a moment to one of his earlier works, a certain insight may be obtained which is denied to us in the article under discussion. In a paper[42] which set a program for much of his later work upon visual perception, Schumann introduces the concept of the 'fusion of sensations to a unity,' an operation which is mediated by the activity of the attention; it is this Müllerian conception which he sets up as an account of *Gestaltqualitäten* in his own terms. In perception, then, we do not deal with the supplementation of ideas to sensations, but we deal with *integrated* sensory contents. For Schumann the problem is simple. We already know by what mechanism sensory contents are integrated; the answer is consistently in terms of the activity of the attention. What remains is the other side of the problem, an analysis of the sensory contents which lie at the basis of the perception of movement, and which are integrated into that perception.

Lasersohn's article contained the last statements made by Schumann on the general theory of the perceiving of movements. Since then the papers published under Schumann's editorship have been purely experimental. In the Fuchs paper we find a series of pretty experiments showing the effects upon the direction of the after-image of movement by the phenomenal character of the projection surface, demonstrating that the after-image is not always perceived in a direction reciprocal

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[37] W. Lasersohn, Kritik der häuptsachlichsten Theorien über den unmittelbaren Bewegungseindruck (Untersuchungen über die Wahrnehmung der Bewegung durch das Auge, Hrgbn. von Schumann), *Zsch. f. Psychol.,* 61, 1912, 81-121.

[38] F. Fuchs, Experimentelle Studien über das Bewegungsnachbild (Untersuchungen, u. s. w., II), *ibid.,* 106, 1928, 267-315.

[39] P. Engel, Tachtistoskopische Expositionen und Scheinbewegungen (Untersuchungen, u. s. w., III), *ibid.,* 107, 1928, 273-313.

[40] K. Steinig, Zur Frage der Wahrnehmung von Zwischenstadien bei stroboskopischen dargebotenen Bewegungen (Untersuchungen, u. s. w., IV), *ibid.,* 109, 1929, 291-336.

[41] A. Blug, Neue Untersuchungen über Scheinbewegungen bei tachistoskopischen Beobachtungen (Untersuchungen, u. s. w., V), *ibid.,* 127, 1932, 290-324.

[42] F. Schumann, Einige Beobachtungen über die Zusammenfassung von Gesichtseindrücken zu Einheiten, *Zsch. f. Psychol.,* 23, 1900, 1-32. A more extended, as well as more sophisticated, treatment of perception from this point of view may be found in G. E. Müller, *Komplextheorie und Gestaltheorie,* 1923, 108 ff.

to that of the originally presented movement. Engel, equating the brightness of the exposure- and fixation-fields of his tachistoscope, comes to the conclusion that movement will be perceived with difficulty unless the two fields are of a considerable difference in brightness. Steinig raises a question answered negatively by Wertheimer, whether one can clearly perceive the intervening phases of a movement. By using pictures of familiar objects, instead of lines and points, he was able to obtain reports of the appearance of the object during its movement, demonstrating that there was not just simple 'awareness' of a movement, but concrete perception of "this object moving from here to there." Blug, in the final paper of the series, extends Engel's conclusions to β-movement, shows that Dimmick's 'gray flash' is dependent upon differences in brightness between exposure and fixation fields, and demonstrates that figures carrying a suggestion toward movement facilitate a perception of movement when tachistoscopically presented, whereas figures carrying no such suggestion do not.

This completes the list of Schumann studies. Upon review, the one fact that is striking about them is the relative minuteness of the problems investigated. We search in vain for an experiment which has some direct bearing upon the basic questions clustering about the problem of the perception of movement. One might almost say of this work that, if you follow Schumann's directions, you will obtain a convenient recipe for a successful arousal of a perception of movement. Nowhere do we find conditions described which may be called necessary. Always the conditions described may be called facilitating or appropriate, but never fundamental. One might say in general of Schumann that he is so interested in experimentation, so desirous of obtaining a 'pretty' demonstration, that, in the case of movement at least, the central problems are almost completely, if not totally, overlooked.

Vittorio Benussi, a student and colleague of Witasek at Graz, published four experimental studies of apparent movement during the six years from 1912 to 1918. The view of perception which Witasek had developed was an integral part of the theoretical equipment which Benussi brought into the laboratory. It is only when this is clearly understood that we can properly estimate his contributions to our problem. First and foremost, Benussi is an adherent to and a supporter of the doctrine of *Vorstellungsproduktion*.[43] Methodologically, this means that he must fractionate his experimental technique: he must, first, determine what is *seen* and, secondly, investigate the conditions of what is *experienced*. For Benussi, this distinction is all-important. It cannot be glossed over without involv-

[43] For the importance of Benussi's work for Witasek's theories, see St. Witasek, *Psychologie der Raumwahrnehmung des Auges*, 1910, 291-338. An extended account of the doctrine of *Vorstellungsproduktion* may be found in Witasek's *Grundlinien der Psychologie*, 1908.

ing the consistency of his systematic position. It is with this distinction as his main weapon that he polemicises against the configurationists. Finally, it is through the application of this distinction that his selection of materials, his treatment, and his conclusions are determined.

Let us glance for a moment at some of Benussi's results. In his first study,[44] working with the Müller-Lyer illusion, he presented successively first the apparently longer, then the apparently shorter, pattern, and observed the expansion-contraction movements later elaborated by Kenkel. To these movements Benussi gave the name of (S) movement, to distinguish them from the (s) movement observed under actual spatial translocation of stimuli. A year later,[45] he demonstrated tactually perceived apparent movement. In his third study,[46] he instituted comparisons between tactually and visually perceived apparent movement, carrying on what he considered to be crucial experiments in support of his views. Finally, in his last paper,[47] he examined the effects of presenting different movements simultaneously in the same visual field, demonstrating movement introduced into stationary objects by moving objects, and many other forms.

A concise statement of his position can be found in a discussion he carries on with Wertheimer at the beginning of one of his experimental papers. He finds that his *Os* are unable to report Wertheimer's objectless movement. Concerning this he says: "Die Erfahrung bietet keine reinen Bewegungserlebnisse im Sinne des Erlebens einer Bewegung ohne Objekt . . . sie bietet nur *anschlauliche Bewegungserlebnisse ohne sinnliche Objektsanschauung.*"[48] Here we see him fitting *phi* into the framework of his distinction between what is 'seen' and what is 'experienced.' This strikes at the core of Benussi's attitude to the problem. Movement for him is never directly apprehended; rather it is an experience, *mediated* by the sense-organs, and turned out by the activity of the mind which we know as *Vorstellungsproduktion.* A summary of his position can be found in the statement that "Die Bewegung selbst wird nicht sinnlich erfasst, wie dass Bewegende an den Ruhestellen, bzw. an den Endstellen der Bewegungsbahn; die Bewegung wird *aussersinnlich* vermittelt."[49]

It can clearly be seen that Benussi's discussion stands and falls with the views of his schoolmaster. Witasek had devoted his academic life to an attempt to bring together, within one systematic framework, the opposition implied in 'act' and 'content.' Such an intention is commendable and the industry with which he applied himself to his task is deserving of praise. Unfortunately, perhaps because of an unavoidable lack of historical perspective, he does justice to neither side of the opposition. His

[44] V. Benussi, Stroboskopische Scheinbewegungen und geometrischoptische Gestalttäuschungen, *Arch. f. d. ges. Psychol.*, 24, 1912, 31-62.
[45] V. Benussi, Kinematohaptische Erscheinungen, *ibid.*, 29, 1913, 385-388.
[46] V. Benussi, Versuche zur Analyse taktil erweckte Scheinbewegungen, *Arch. f. d. ges. Psychol.*, 36, 1917, 59-135.
[47] V. Benussi, Über Scheinbewegungskombination, *ibid.*, 37, 1918, 233-282.
[48] V. Benussi, Versuche zur Analyse taktil erweckte Scheinbewegungen, *ibid.*, 36, 1917, 68 (italics added by the present writer).
[49] *Ibid.*, 70.

basis is openly sensationalistic, upon which he erects a superstructure of mental acts and agencies. Although he was aware of certain of the inadequacies of the doctrine of sensations, he was not able to make a decisive break with the dogma, and tried to bridge the gap by positing a series of hypothetical mental activities. Demolish his sensationalistic base, as Köhler[50] and others have done, and his entire system collapses. It is by reason of his intellectual dependence upon Witasek that Benussi's studies of movement form another group of those which have left little impress upon the general character of the problem as it stands today.

With the publication of the study of Johannus Wittman,[51] who had obtained his training and conducted his experiments under Götz Martius at Kiel, we find the routine phenomena of movement treated from a point of view quite different, in some respects, from those discussed so far in this survey. Along with the Wundtian tradition an opposing movement had been developing, to which the work of Dilthey had given impetus, and which had been concretized by Martius, Wittman's teacher. The essentials of this movement consist in a denial of the analytic-synthetic method of the Wundtians and a substitution in its place of a developmental account of mind. The problems of psychology, then, are two; (1) how is this present perception (thought, action) to be accounted for biographically, and (2) what are the present conditions of its arousal? These problems may be investigated independently of each other, but a full account of a given phenomenon involves both.

Wittman restricted his investigations of movement to the second of the two problems mentioned above, which may be re-stated as follows. Given the developed perception of movement in the adult individual, what are the conditions, subjective and objective, of it arousal? This is his problem. His background we have already discussed. With such an equipment and with such an aim, we might well expect to find a straightforward, simple and—providing his controls were adequate —a valuable exposition of the conditions of this perception. Unfortunately, that is not what we find. Instead, the doctrine of synthesis, which Martius had attacked, appears again in the work of his student. Speaking of movement, Wittman declares that "das Erlebnis des Sehens von Bewegung zwischen irgendwelchen sukzessiv gebotenen und *ebenso erfassten Sehdingen* lässt sich . . . als ein Zusammenhangserlebnis bezeichnen."[52] Wittman speaks of *Sehdinge,* visual objects, falling into the now

[50] For an extended and expert critique of the theories of *Vorstellungsproduktion,* see Wolfgang Köhler, Über unbemerkte Empfindungen und Urteilstäuschungen, *Zsch. f. Psychol.,* 66, 1913, 51-80. The difficulties besetting theories of the type of Witasek's will be discussed more fully in a later section of this paper.

[51] J. Wittman, *Über das Sehen von Scheinbewegungen und Scheinkörpern,* 1921, 204. For a brief statement of Martius's views, see G. Martius, Über synthetische und analytische Psychologie, *Ber. u. d. 5. Kongr. f. exper. Psychol.,* 1912, 261-281.

[52] *Ibid.,* 16, (italics added).

ancient error of an identification of his two stimulus-energies with two seen objects. Making this fatal error, his next move is inevitable. If the perception of movement is simply the experience of a certain relationship between *two* visual things, we have only to find the peculiar temporal and spatial relationships in which the visual things must be found in order that movement may be experienced between them.

This unfortunate formation of the problem restricts the importance of Wittman's experimental results. Since he was concerned with the discovery of the temporal and spatial relationships between two visual objects, we find him beginning—of course—with a systematic variation of the temporal interval. Like Wertheimer, he finds certain stages or states of movement, as we pass from simultaneity to succession; but he outdoes the *Gestalt* investigator and finds eight instead of three. Furthermore, since movements are . . . "Gesamtvorstellungen, die aus einem Zusammenschluss von Einzelvorstellungen, von Einzelbildern . . . hervorgehen,"[53] the direction of the observational attitude is important and takes equal rank with the temporal interval. Here we find him distinguishing an analytic from a synthetic attitude, the former inhibiting, the latter facilitating, a perception of movement.

Two other things develop from Wittman's inadequate statement of the problem. First, he summarily denies the existence of *phi,* Wertheimer's objectless movement. If, as Wittman regards it, the perception of movement is an experienced relationship between two *objects,* then to speak of objectless movement is to utter an absurdity. Contrary to this opinion, we have the evidence of numerous investigators that this variety of abstractive perceiving can and does occur, if the conditions are suitable. Secondly, he claims that movement may be perceived without an identical object moving, developing his conception, as Wertheimer had done before him, from his analysis of movement into various stages. At the same time he admits—just as the configurationist had—identity in the optimal state. We must refer the reader to the objections urged against the comparable view of Wertheimer; the criticisms mentioned there are just as much in place here.

Wittman's experimental work, conclusions, and inferences are disappointing when the comparative novelty of his introductory remarks is remembered. His experiments are casual, and often relatively uncontrolled. The emphasis which he places upon the direction of the observational attitude may well derive from the fact that the temporal intervals with which he worked appear to be too long to present the most adequate conditions for the perception. His eight stages of movement contribute no more to the problem than Wertheimer's three, and his mentalistic treatment is open to the difficulties of all such views of perceiving.

The next important study in this field is that of Franz Hillebrand,[54] who treats his problem in a manner which has at least one repercussion in later work by other men.

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[53] *Ibid.,* 81.

[54] F. Hillebrand, Zur Theorie der stroboskopischen Bewegungen, *Zsch. f. Psychol.,* 89, 1922, 209-272; 90, 1922, 1-66.

First and foremost, Hillebrand is interested in physiological optics, standing in the same relation to Hering as Von Kries stood to Helmholtz. He is interested only secondarily in the problem of movement, attempting to bring the movements observed under successive retinal stimulation into line with the typical spatial perceptions dealt with in his chosen study. He begins with the circumstance, familiar to students of physiological optics, that one may sweep one's eyes over a physically stationary object and perceive no movement, in spite of the undoubted fact that the physical image has shifted across the retina. Against this he opposes the contrasting fact that one may perceive movement in the absence of retinal shift, as when following an object with the eyes (pursuit movements). It seems obvious, then, that there must be factors other than retinal which contribute to the arousal of a perception of movement. As further evidence for this supposition there are the facts of stroboscopic movement, where movements may be perceived with neither eye-movements or retinal shift operating as conditions. To account for these non-retinal factors he develops an idea first suggested by Hering, which has become known as the theory of 'absolute localization.' According to this doctrine, there are certain central space-factors, typically discussed under the categories of attention, which may compensate for changed retinal space-values. These central factors construct the visual field in which the object is seen; the perceived position of an object is accounted for by the algebraic sum of retinal and central space-values.

With these terms defined, Hillebrand turns to the problem which forms the principle subject-matter of his paper. The situation is familiar. Immediately or shortly after spot *a* of the retina is stimulated, spot *b* is excited. Spot *b* may lie at a considerable distance from spot *a* and the temporal interval may range from zero to as long as a second or two. Under such circumstances, *O* will report movement where no continuous retinal shift has occurred and where there is no movement in the physical world. For Hillebrand, this presents an anomalous, but not inexplicable, case under the general question of retinal and absolute localization. His whole case centers about the statement that stroboscopic movement is not perceived until the second stimulus is actually effective upon the retina.[85] Under such circumstances, retinal area *b* undergoes a gradual revaluation (*Umwertung*) of its space-value; under influence of absolute localization, the visual field gradually shifts from a position centering around *a* to one around *b*. The psychological correlate of this gradual process of revaluation is the perception of movement.

What is there to say of Hillebrand's contribution? In essence, he maintains that the whole process is explicable in terms of certain relationships between two antagonistic processes, retinal localization and absolute (or attentional) localization. Here, in the assumption of two antagonistic processes, we come to the central point of his difficulty—a confusion which is implicit in much of the work in physiological optics. If one begins a study of the perception of space with the perfectly natural belief in a certain correspondence between retinal and perceptive events, one immediately encounters a great mass of phenomena which seem to diverge markedly from such a rule. It appears to be the exception and not the rule which prevails. The perception of moving objects appears to fall most readily under the class of exceptions, and especially those perceptions of movement which are

[85] F. Hillebrand, *op. cit.*, 89, 1922, 233 ff.

obtained under discrete stimulation. In attempting to account for these discrepancies, however, Hillebrand makes his error with reference to the retinal events with which he begins his argument.

If we confine ourselves to those particular stimuli—selected from the mass of those effecting the retina at the moment—which represent the object in the visual field perceived as moving or stationary, then it is obvious that, during pursuit movements, these stimuli do not shift across the retina. But what of the other stimuli, those which correspond to *Grund* (if those previously mentioned correspond to *Figur*)? Is it true that these are not undergoing a shift across the retina during pursuit movements? Just as obviously as the previous case was true, this is untrue. There is always some part of the total constellation of stimuli shifting across the sensitive surface of the retina during pursuit movements of the eyes. Hillebrand makes the fatal error of confining his discussion to only one part of the total retinal stimulation at a given time, whereas a consideration of the total stimulation would have changed the entire character of his problem. But what of the case of movements perceived under discrete stimulation? With the eyes held stationary, can there be a shift of stimulation? Hillebrand to the contrary notwithstanding, we must answer this question in the affirmative. Granted that there is no *continuous* shift across successive retinal elements, we still must insist that there is a *change* in retinal stimulation, and, furthermore, that it is a *spatial* change. The most simple statement of what occurs might be as follows. Where certain temporal and spatial limits are not transcended, the organism performs in exactly the same way whether the stimulation at the moment is discrete or continuous. It must be emphasized that these limits are subject to the modification of a host of variables, many of which are indicated in the experimental researches upon this topic (see our summary below).

With a consideration of the possibilities indicated above, the necessity for a cumbersome system of antagonistic processes, such as Hillebrand assumes, simply disappears. The strong possibility remains that we may begin our account of the perception of movement with certain neglected aspects of the stimulating agencies, provided that we extend our treatment from a consideration only of the two 'principal' stimuli to that of the total retinal stimulation at the moment, and provided our knowledge of a large number of other variables is sufficiently profound.

Hillebrand's treatment was most influential upon the researches of two Dutch neurologists, H. G. van der Waals and C. O. Roelofs of Amsterdam.

In a series of articles[58] they subject to an exhaustive scrutiny the phenomena of movement observed under discrete stimulation, and come to the same conclusion at which Hillebrand had arrived; namely, that the problems of movement are part-problems under the more general problem of localization. They arrive at this con-

[58] H. G. van der Waals and C. O. Roelofs, Optische Scheinbewegung, *Zsch. f. Psychol.*, 114, 1930, 241-288; 115, 1930, 91-193; Über das Sehen von Bewegung, *ibid.*, 128, 1933, 314-354.

clusion in quite a different fashion, and the novelty of their view merits discussion.

Just as Wertheimer had done before them, they begin by varying the temporal relations of two stimuli. They report the appearance of eight stages, from phenomenal simultaneity to phenomenal succession. Of prime importance for them is not the stage of 'optimal movement,' where only *one* object is seen moving in the field, but those other phenomena, also emphasized by Wertheimer, where two visible objects are undergoing movements independently of each other. Instead of using Wertheimer's *Teilbewegung* and *Singularbewegung* as descriptive terms, these investigators develop a terminology of their own. They use the term *Auftauchbewegung* (appearance-movement) to describe the case where the second object seems to spring from the direction of the first, while the first is still visible; *Erlöschbewegung* (disappearance-movement) is used where the first stimulus is removed while the second is still present, the former seeming to disappear in the direction of the latter. From consideration of the limits of possible variation, they come to the conclusion that exposure-time is a more important determiner than the temporal interval. Furthermore, they observe that fixation at the place of stimulus *b* tends to break down a perception of movement, that fixation at spot *a* tends to favor its arousal, that fixation upon a point midway between the two is neither as good as the second case nor as bad as the first. Combining the results of their preoccupation with exposure-time and fixation, they come to the conclusion that "das Sehen von Scheinbewegung ein Problem der relativen Lokalization der betreffenden Objekte während der Bewegung ist."[57]

Feeling, possibly, that their general conclusion was not quite as obvious as it might be, they undertake a more extended discussion of it in a later section of their paper. Deviating somewhat from Hillebrand's treatment, they also distinguish two separate components in the total process; these are (1) the component at the basis of the light-sensation itself and, (2) the component which fixes the visual localization of the light-sensation. The first of these is a peripheral, the second a central, process. The importance of their 'appearance' and 'disappearance' movements is now demonstrated. They are interpreted as follows. During the succession of optical stimuli, the localization in which the second (first) stimulus appears—dependent of course upon the exposure-relationships—is influenced by the localization of the first (second) stimulus. The movements observed are nothing other than "die Übergang dieser abweichenden in eine weniger abweichende, bzw. normale Lokalization."[58] For these investigators, then, the situation becomes simple. If the intensive relationships between the two psychophysical processes corresponding to the localization of the two stimuli are of a certain sort, then there occurs a fusion of the disappearance-movement of the first stimulus and the appearance-movement of the second stimulus; when such a fusion occurs *O* reports the perception of a smooth movement of a single object.

The arguments of these Dutch workers are at once more naïve and more subtle than those of Hillebrand. Their discussion is less involved than that of the Austrian investigator, but their conception of movement as a synthesis of two part-movements includes an error which Hillebrand had

[57] Van der Waals and Roelofs, Optische Scheinbewegungen, *ibid.*, 114, 1930, 271.
[58] *Ibid.*, 166.

managed to avoid. This fusion of two movements into one is open to the same criticisms put forward against the exponents of 'mental chemistry.' The fact that this present formulation is clothed in a more sophisticated terminology—the terminology of modern physiological optics—cannot in itself prevent the theory from attack on the grounds of an old weakness. Concerning the discussion of the problems of movement under those of localization, reference may be made to the criticisms of Hillebrand in preceding paragraphs. Extensive criticism of Van der Waals and Roelofs with regard to this basic point would involve simple repetition of what has already been said.

By far the best study so far published in this field is that of W. Neuhaus,[59] a student of Ach at Göttingen. His analysis of the difficulties inherent in the experimental techniques involved in all the studies preceding his is so complete that his own methodology is beyond criticism. His results, largely phrased in terms of the stimulus-conditions for the perception of movement under discrete stimulation, must be taken as final. On the side of theory he is not quite so fortunate.

Neuhaus believes that the chief experimental error at the basis of most of the suggested theories is that of insufficient variation of the stimulus-conditions. The still unsettled controversies about the importance of such conditions as temporal interval, exposition-time, and distance between the stimuli, rest, according to Neuhaus, upon the circumstance that only small sections of the possible ranges within which these variables find their effectiveness have hitherto been investigated. He proposes, therefore, to investigate these conditions within as wide as possible a range of variation.

Using apparatus which permitted such variation, as well as a large number of trained *Os*, be obtains certain significant generalizations: (1) If exposition-time (E) and distance between the stimuli (d) are kept constant, variation of temporal interval (T) has decisive influence upon the arousal of a perception of movement. There is, however, quite a large range within which variation of T has little or no effect, *e.g.* if $E = 10\ \sigma$ and $d = 7.5$ cm., movement is perceived with T ranging from 80 to 400 σ, but breaks down outside of these limits. It must be understood that these figures are not absolute, but are relative, changing with certain changes in E and d. (2) If E is constant and T and d are varied, increase of d requires increase of T. Increase of E will permit a certain increase of d over and above that permitted by increase of T. In general the smaller d is, the more easily will the perception of movement be aroused. (3) If E and T are varied and d is held constant, it is found that a short E requires a long T, and conversely, although the values are not inversely proportional. Further, if E_1 represents the exposure time of the first stimulus and E_2 that of the second, then variation of E_2 has little effect upon the phenomenon, while variation of E_1 has the same effect as if both were varied equally.

[59] W. Neuhaus, Experimentelle Untersuchung der Scheinbewegung, *Arch. f. d. ges. Psychol.*, 75, 1930, 315-458.

(4) Movement is obtained very readily with temporal over-lap (negative values for *T*), but only if *E* is larger than a certain minimal value. (5) Variation of the physical intensity of the stimuli necessitates no change in any of the other variables. Movement may be obtained just as well with differently-colored as with equivalently colored lights, with lights of differing intensity as with lights of the same intensity. This is true within fairly wide ranges.

Having investigated certain of the most important stimulus-conditions, he now turns his attention to certain significant variables inherent in the experimental situation. Under the general heading of 'the development of apparent movement,' he takes up the important question of the degree of dependence of the perception upon its stimulus conditions. He states very emphatically that the relations of dependence which he had established *hold only for trained Os*. He notes that conditions which are adequate for trained *Os* will typically arouse perceptions of simultaneity or succession in *Os* unpracticed in perceiving movement under conditions of discrete stimulation. During the first part of a training period, *Os* consistently report no movement; reports of movement suddenly make their appearance and *Os* often will announce surprise at the appearance of an event which had not been perceptible before. Neuhaus also tells us that the effect of instruction is important; a formal instruction to see movement will arouse its perception in *Os* who have hitherto never reported it. Again, as the period of practice is lengthened, movement may be perceived over a wider and wider range of variation in the extraorganic conditions. All these facts lead Neuhaus to argue against those theorists—principally the configurationists—who contend that the perception is solely dependent upon the stimulus-constellation. For Neuhaus, central factors, designated by such terms as 'attitude,' 'set' and 'predisposition,' play important rôles in the perceiving of movement under discrete stimulation.

With all his data in hand, Neuhaus completes his study by making certain statements which are of significance for theory. He recognizes, as others had done before him, that apparent movements and 'real' movements are, within certain ranges, phenomenally identical. The central differences between the two must be found in the conditions of their arousal. For Neuhaus, the way is shown to an important condition by those factors which he discusses under the heading of 'development.' These indicate to Neuhaus that the fundamental determiners of the perception of apparent movement are central events, that there is here a certain independence of the character of stimulation. From these correct statements Neuhaus manages to draw a somewhat unfortunate conclusion. We find him saying "Es ist also die Verschmelzung der Reizkomplexe zu einer Einheit, zu einem Gegenstande die conditio sine qua non der Entstehung der stroboskopischen Bewegung,"[60] a formulation which differs in no way from that of Schumann (cf. pp. 17-18), also Göttingen product, so many years before.

For all Neuhaus's critical attack upon the conditions of the perception, he is not quite able to free himself from the doctrine of 'mental chemistry,' an error against which Wertheimer had wisely polemicised two decades before.

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[60] W. Neuhaus, *op. cit.*, 453.

Before leaving our survey of the articles on movement which have appeared in the German journals, we must make brief mention of two papers which are interesting chiefly because of the novelty of the conditions investigated.

Heinz Werner published a study on movement as one of the long series on the 'structure' of perception which have appeared under his editorship. The question he puts is simple. What is the effect upon the perception of movement by way of discrete stimulation if, simultaneously with the visual stimuli, certain rhythmic stimulations occur in other sense-departments. In collaboration with Zeitz, a student of his, he puts the question to experimental test,[81] trying the effects of auditory and kinesthetic rhythms of various types and rates. His results while not surprising, are interesting. He uses a rate of succession of the visual stimuli which is below the optimum for the perception of movement; if, at the same time, an auditory rhythm is presented, composed of clicks at a rate slightly faster than that of the visual stimuli, his *O*s now report definite movements in the visual field. He obtains the same results for kinesthetic rhythms of various types. For Werner these results indicate that the perception is not a purely visual, or purely auditory, performance, but is a total performance, involving the whole organism. The value of Werner's contribution is in the direction of a demonstration of the intimate interrelation which exists between the contributions of many sense-departments to any perception. It is more correct to speak of an apprehension of movement *under visual or other stimulation* than of a visual perception of movement.

The second article which is distinguished by its novel conditions is that of P. A. Galli, of the University of Milan, a student of Gemelli and Wertheimer. He proposes the interesting variation of drawing his successive excitations from different sense-fields.[82] Thus he may present his stimuli as follows; optical-tactual-optical, or optical-optical-tactual, or again tactual-optical-optical. He may vary the relationships still further by using the following combinations; tactual-auditory, optical-auditory, and optical-tactual-auditory. Varying the spatial and temporal relationships of these combinations of stimuli he obtains the most striking apparent movements. His *O*s report a particular visual object moving in the direction of a pressure, a pressure moving in the direction of a sound, and so on, for many combinations. Galli makes little attempt to discuss the implications of his data with respect to the theories of the perception of movement. To us they indicate the functional character of the perception of movement, as will presently be discussed more fully.

IV. American Investigators (1920-1934)

The earlier studies upon this problem performed in this country took the form of polemics against only one group of those so far discussed. The theories of *Gestalt* had become, or were about to become, crucial issues in the discussions of American psychologists. It is not at all surprising, then, that the first investigations of movement concerned themselves with problems raised by Wertheimer's 1912 article, as well as by those papers published by his followers during the next few years.

[81] H. Werner and K. Zeitz, Über die dynamische Struktur der Bewegung, (Studien über Strukturgesetze, VIII), *Zsch. f. Psychol.*, 105, 1928, 226-249.

[82] P. A. Galli, Über mittelst verschiedener Sinnesreize erweckte Wahrnehmung von Scheinbewegungen, *Arch. f. d. ges. Psychol.*, 85, 1932, 137-180.

It was E. B. Titchener, then head of the Cornell Laboratory, who first set a graduate student the task of examining Wertheimer's contentions. Those familiar with Titchener's systematic position can readily understand that certain of Wertheimer's statements would be very repugnant to him. To a psychologist who was convinced above all others that the sole task of psychology was the analytical investigation of 'mental contents,' such a contentless perception as Wertheimer's 'pure *phi*' could not go unchallenged. Wertheimer had made the claim of complete unanalyzability for *phi;* it was spoken of as an 'experience of movement *sui generis*.' For Titchener the problem was clear; either *phi* is a new sensation, a new element of mental experience, or else, treated properly, it will admit of analysis. The task was given to F. L. Dimmick "to put this elementary movement-experience, this phi-phenomenon, under critically descriptive conditions, and to test its unanalyzability."[63]

Closely following Wertheimer's technique, Dimmick's only innovation is to control the attitude of observation by appropriate instruction. To point the distinction which Titchener had made between 'context' and 'process,' Dimmick instructed his *O*s to give two reports at each stage of the experiment, one in terms of 'meaning,' the other in terms of 'process.' Working under the first of these instructions, they were simply to report upon what happened in the field of vision; in the second, they were to report upon the sensations and to describe these in as much detail as possible. In general, Dimmick found that wherever movement was reported (meaning-attitude), the process-report is in terms of a 'flash of gray.' He emphasizes that this gray is not "the gray of a piece of paper. The temporal attribute is beaten up with the quality into an integration of the first order . . . *O*s describe the experience as a 'flashing' . . . 'shimmery' . . . 'live' gray."[64] It is this 'gray flash' then, this "integration of protensity and quality,' which is the existential correlate of stroboscopic movement.

The later vicissitudes of this conception have been various. The contention must face the serious charge that the 'gray' is an artifact of particular experimental conditions. Blug, working under Schumann, claims that the 'gray' is simply not observable if the preëxposure field and the exposure-field are equivalent in brightness, arguing that the observed quality is a function of the dark *Zwischenfeld* used in Dimmick's experiments.[65] Other men[66] insist that movement may be perceived very many times where no process-report in terms of quality can be obtained from *O*. In addition to these cogent criticisms another, more general, objection might be raised, which has not appeared in the experimental literature. What is

[63] F. L. Dimmick, An experimental study of visual movement and the phi phenomenon, this JOURNAL, 31, 1920, 317-332.
[64] *Ibid.*, 331-332.
[65] A. Blug, *op. cit.*, 317 ff.
[66] Higginson, McConnell, Van der Waals and Roelofs, Neuhaus, *et alii*.

the relation, it might be asked, between some such visual quality as Dimmick suggests and the perception of movement. Obviously this intermediate 'gray' serves to fill the gap between the two excitations; it guarantees the continuity of the movement. What is the difference between this suggestion, couched in more sophisticated terms it is true, and the traditional explanations (Linke, Benussi, Wittman) in terms of a supplementation of a central 'idea of movement' to the two 'sensations.' Obviously there is very little difference. Both these explanations try to account for the apparent discrepancy between the perceived continuity and the stimulus discontinuity by some central filling-in process. Both are open to the objections which may be urged against all 'mental chemistry.'

Six years after Dimmick's article, the second major attack upon Wertheimer's results appeared in a study directed by Madison Bentley and carried out by G. D. Higginson at the University of Illinois.[67]

Realizing the paramount significance that the temporal interval possessed for the German investigator, Bentley proposed a more critical investigation of the dependence of the perception upon this variable. Using a range of intervals from 15 to 225 σ, with a constant exposure-time of 45 σ, Higginson obtained reports from his *O*s indicating a complete *lack of dependence* of the perception of movement upon length of interval. Examination of his tables discloses as many reports of full movement at the smallest interval as at the largest. The same lack of dependence is demonstrated for the other phenomena reported by Wertheimer (*Teilbewegungen, Singularbewegungen,* etc.).[68]

Examination of the instructions under which his *O*s worked and perusal of the sample reports found in the article indicate that the *O*s were to report movement however derived. We find Higginson saying, "It must not be assumed that 'full movement' always meant that a line was apprehended to move, as a line, across the field . . . the *O*s reported at times that the experience *meant* that one line moved from one position to another; but that the actual movement of a line . . . was not seen."[69] Other workers imply, for the 'optimal' intervals, a clear movement of a single object; but it is difficult to be certain that their instruction to, and training of, *O*s would have brought out the differences which Higginson finds in his full reports. A more important aspect of Higginson's study is suggested by his declaration that "we have here clearly demonstrated the remarkable facility of the organism whereby it may attain a functional end under extreme poverty of experiential resources."[70] Here we have an indication of the influence of 'set,' 'predisposition,' and 'tuning' upon perceiving, an influence which had received too little treat-

[67] G. D. Higginson, The visual apprehension of movement under successive retinal excitations, this JOURNAL, 37, 1926, 63-115.

[68] It is unfortunate that Higginson omits his visual angle since Neuhaus has since shown (*op. cit.*, 337 ff.) that this angle tempers the effect of the time-gap between stimuli.

[69] G. D. Higginson, *op. cit.*, 79.

[70] *Ibid.*, 111.

ment in the studies prior to Higginson's. The most striking thing about the perception of movement under discrete stimulation is that the organism is performing in a particularly impressive way under extra-organic conditions which appear inadequate. Higginson is perfectly correct in casting doubt upon the absolute effectiveness of the temporal interval. Unfortunately, he leans too far in the other direction, though his study affords some insight into the various ways in which so complex an organism as the human adult may perform under a rigidly fixed arrangement of bare, resting ocular excitations.

A study which takes its point of departure from Higginson's investigation is that of R. F. McConnell, also from the Illinois Laboratory.[71] It continues the study of the temporal interval.

Higginson had obtained reports of movement where the temporal interval was zero and the exposition-time as long as 5 sec. McConnell asks whether or not "movement is similarly obtained under excitations which are *contemporaneous* and *very brief?*" Her procedure was simplicity itself. To produce a terminal overlap, both stimuli were presented at the same time, but one disappeared a variable length of time before the other. To produce an initial overlap, one stimulus was presented a variable length of time before the second, but they disappeared together. The typical stimulus-figure was two straight lines in an oblique-horizontal direction, forming an angle of 45°. The apparatus was the same as that used by Higginson, with slight modifications. McConnell's results are clear and conclusive. Where the overlap, either initial or terminal, is small, very few reports of movement occur. The frequency of the 'movement' report increases rapidly as the degree of overlap is increased, to reach a maximum about a third of the way through the range. This maximum is maintained, with occasional inversions, up to extreme overlap. As McConnell formulated this result, "the frequency of phenomenal movement under simultaneous excitations bears a direct relationship to the *amount* of overlap of one of the members of the stimulus object."[72]

What bearing have these results? Examination of McConnell's protocols convinces us that she is dealing with temporal disjunction of excitations as all the other workers in the field have done, but it is temporal disjunction of a peculiar sort. Until this time, 'temporal interval' had meant a certain 'empty' time between the successive visual stimuli which were supposed to determine the apprehension of movement. With McConnell's overlap this time gap did not appear. Instead, one excitation outlasted (at beginning or end) the other. There is a kind of temporal disjunction, but it is not the kind implied by those who had held the time-gap to be of primary importance for perceiving movement.

The importance of McConnell's work is apparent. She was the first to point to a fact which Neuhaus and Van der Waals and Roelofs later confirmed, *i.e.* that the temporal coefficient which serves as an extra-organic condition of the perception is a complex variable. Her work adds to the

[71] R. F. McConnell, Visual movement under simultaneous excitations with initial and terminal overlap, *J. Exper. Psychol.*, 10, 1927, 227-246.
[72] *Ibid.*, 245.

accumulated evidence against the configurationist's dogma of the temporal interval.

Since 1926 H. R. DeSilva has published three articles upon the perception of movement[73] and throughout he borrows freely from any or all doctrines which seem to him to present possible explanations of his results. If any view is dominant it is that of 'mental chemistry,' although he is rarely consistent in its application. Attention —used both as activity and as process—plays a large part in his discussions, such phrases as 'clearing-up in consciousness' being common. Completely uncritical—as if the work of the last twenty years simply did not exist—he rests his case upon such outworn distinctions as those between sensations and perception, between subjective and objective determinants, and so on. It must be emphasized that these do not represent merely differences of emphasis, as they have for other men. DeSilva uses them as mutually exclusive categories, although he throws a sop to the reader by calling them heuristic principles.

In his 1926 article, DeSilva sets out "to discover the principal determinants or conditioners of this phenomenon of visual movement . . . the factors of repetition, fatigue, background, position, form of stimulus, meaningfulness of stimulus, fixation-point and predispositions of various sorts."[74] Accepting Wertheimer's figures, he presents what he believes to be the optimal conditions for the perception, an exposure-time of 65 σ and a temporal interval of 60 σ. He says nothing of the prevailing distances between his stimuli, allowing the reader to guess whether they were adequate or not. Allowing for the fact that his *Os* appear to be relatively untrained, he obtains much the same results as others have obtained before him. Like Wittman he differentiates between an 'analytic' and a 'synthetic' attitude, for the reason that his *Os* now report movement, now do not, under the same stimulus-conditions. Of the other variables, we find his results discussed, usually, in terms of 'degrees of clearness,' although he does not state the criteria upon which these reports were based. Largely upon the basis of the great variability which he obtained in his reports, he concludes that "the results emphasize the tremendous importance of subjective determinants."[75]

Before we examine DeSilva's contentions, let us see what he has to say in his last investigation. In addition to the dubious distinctions made in his earlier study, he now finds it necessary to make several more. First of all, he distinguishes between 'movingness,' the pure experience of movement comparable to Wertheimer's *phi*, and the 'vehicle-of-movement,' the object seen in movement. A further distinction is made between 'objective facts' and 'psychic determinants;' under the latter he discusses the "functions of attention or its product, the clearness aspect of consciousness." Presumably, each perception involves both movingness and vehicle-of-movement, the character of the perception being determined by the different levels of clearness of these two entities.

We begin our appraisal of DeSilva with the most concrete and revealing general

[73] H. R. DeSilva, An experimental investigation of the determinants of apparent visual movement, this JOURNAL, 37, 1926, 469-501; Kinematographic movement of parallel lines, *J. Gen. Psychol.*, 1, 1918, 550-575; An analysis of the visual perception of movement, *Brit. J. Psychol.*, 19, 1929, 268-305.

[74] H. R. DeSilva, An experimental investigation of the determinants of apparent visual movement, this JOURNAL, 37, 1926, 469-470.

[75] *Ibid.*, 501.

statement which he makes: "The illusion of movement from an apparent movement situation . . . involves a 'filling-in' not by 'sensory' short-circuiting processes, which take place presumably in the lowest centers of the response arc, but by central 'perceptual' processes which take place in the highest centers of the response arc."[76] If we add to this statement his use of such terms as 'attention' and 'clearness,' and remember his fractionation of the perception into 'movingness' and 'vehicle-of-movement,' we see that DeSilva's theory of perception is in terms of a very complex variety of 'mental chemistry,' and that it carries to the fullest expression the evils of such mentalistic theories. Like Marbe, Hillebrand and others, DeSilva chooses 'attention' as the special agency of mind. DeSilva's function of attention offers the following service in the perception of movement: "All illusions of apparent movement . . . seem to involve . . . sensory over-lapping or succession of 'clearing-up' processes on neighboring retinal areas, and a contemporaneous integration of at least two spatially differentiated but similar central processes which possess similar degrees of effective 'clearness.'"[77] The ghost of attention refuses to be laid. Rather, as the evidence in support of this concept decreases in cogency, attention seems to become more and more the be-all and end-all of psychological activity. DeSilva's ponderous and labored explanations will be taken up in our closing section, in connection with others of the same sort.

DeSilva's studies represent the last attempt by an American at an extended investigation of the more important variables involved in this problem. The four or five papers on apparent movement which appear after 1926 (excluding the long paper published in England by DeSilva) are devoted to treatments of some small facet of the problem which had escaped earlier investigation or which did not appear to be wholly settled. So we find two papers from the Princeton laboratories, under the direction of H. L. Langfeld, one a brief study of phenomena noted at a temporal interval of 60 σ,[78] and the other a report upon the successful verification of haploscopically perceived movement.[79] Two years later, Guilford and Helson photographed eye-movements during observation of apparent movements,[80] disproving, in part, Higginson's recent contentions. Again two years pass and we find Squires devoting himself to the use of colored stimuli, obtaining clear reports of 'colored' movements.[81] Finally, in the same year, Mibai,[82] working under Pillsbury at Michigan, contributes a long paper, the most important part of which is concerned with a determination of the conditions of the apparent speed and direction of stroboscopic movements. With Mibai's research we come to the end of the long journey which we had started with Exner. We shall now try to summarize our review.

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[76] H. R. DeSilva, An analysis of the visual perception of movement, *Brit. J. Psychol.*, 19, 1929, 292 (note 1).

[77] H. R. DeSilva, *Brit. J. Psychol.*, 19, 1929, 303.

[78] H. G. Vogt and W. Grant, Jr., A study of the phenomenon of apparent movement, this JOURNAL, 38, 1927, 130-133.

[79] H. S. Langfeld, Apparent movement with a stationary stimulus, this JOURNAL, 39, 1927, 343-355.

[80] J. P. Guilford and H. Nelson, Eye-movements and the phi-phenomenon, *ibid.*, 41, 1929, 595-606.

[81] P. C. Squires, The influence of hue upon apparent movement, *ibid.*, 43, 1931, 49-64.

[82] S. Mibai, An experimental study of apparent movement, *Psychol. Monog.*, 42, 1931, (no. 190), 1-91.

V. Summary and Conclusions

We are at last in a position to make certain fundamental statements about this problem which has been variously attacked by many men. There are certain basic questions which should be asked and which deserve careful consideration. First and foremost, we must answer the question as to the particular array of experimental facts about which there has been general agreement, or at least has aroused no serious opposition. Secondly, we should indicate those experimental results about which there is still serious disagreement, and try to indicate some way in which resolution of the difficulties may be attempted. Thirdly, we should attempt to bring some order into the chaos of apparently conflicting theories which has so befogged this field of research. Finally, we should present a few words of a more general nature upon the character of the problem as it appears in the light of recent research, attempting to anticipate certain of the directions which future work may profitably take.

First of all, then, the facts. What are the conditions, generally agreed upon, which make for a perception of movement? It will be convenient to arrange these results of prolonged experimentation in some sort of classification, since the conditions found are many and diverse. The following outline represents an attempt to bring all the significant results under one set of rubrics, a presentation which will make for economy and clarity of discussion, although it does some violence to a logic of mutually exclusive categories.

(1) Extra-organic variables

(a) Temporal relationships

(i) Time-interval. Where stimuli have been separated by a time-gap, there has been a general agreement that the interval is fundamental. Agreement upon the quantitative limits has not been quite so complete. Van der Waals and Roelofs and, especially, Neuhaus have done the best work here, effectively combating Wertheimer's dogma of 60 σ, showing the inadequacies of Korte's 'laws,' and demonstrating the reciprocal dependence of length of interval upon other variables inherent in the situation.

(ii) Exposure-time. The length of exposure of individual stimuli first received treatment by the configurationists (cf. Korte), but considerations of technique, as well as theory, caused them to minimize its importance. Later, Hillebrand, Van der Waals and Roelofs, and Neuhaus present data demonstrating this variable to be of decisive importance, a change in exposure-time often requiring certain changes in the other variables. Neuhaus is the best reference.

(b) Spatial relationships

(i) Distance between stimuli. Extended treatment by Korte and Neuhaus. Within wide limits, distance and interval should be varied directly. The work of Neuhaus indicates that visual angle ranks with interval and exposure-time as a fundamental variable. To state stimulus-conditions without mentioning it is to risk inadequate and deceptive data, an error implicit in much of the work in this field.

(ii) Form. Linke, certain of the configurationists, and the Schumann group have all presented evidence that the direction, speed, course, and 'goodness'

of the movement perceived are dependent upon stimulus-configuration. (See Linke's *Bogenversuch* and the three-dimensional movements reported by Wittman, Steinig, Koffka and others.)

(iii) Size. Good expansion-contraction movements have been reported by many observers where one stimulus is of larger physical or apparent size than the other. The limits of difference have never been reported, although this question has a certain theoretical, as well as practical, importance. No relationship has been established between size and the variables discussed above.

(c) Intensive relationships

(i) Absolute intensities. Korte reports that increase of the absolute intensities requires increase in the distance factor and decrease of temporal interval. This relationship has been broken down by Neuhaus, who used even greater intensities than Korte had used and obtained no such effect. A decisive answer upon this question requires more extensive investigation.

(ii) Relative differences in intensity. An impressive number of investigators agree that intensive differences have no effect upon the phenomenon other than that there will be a decrease and increase in brightness during the course of the movement. Korte reports movement in a direction opposite to the temporal sequence of the stimuli when the second stimulus is more intense; this δ-movement has suffered from lack of adequate verification.

(d) Character of stimulus

(i) Color. A large number of men have worked with stimulus-energies of different wave-lengths. Most report that the object appears to change color during the course of its movement. There has been no final study of the qualitative limits nor a precise enough description of the types of change occurring during the movement. This topic merits further experimental treatment.

(ii) Insistence. By many devices (*e.g.* fixation, increase of intensity, change of form) men have attempted to determine the effect of making one stimulus more insistent than the other. On this question there is disagreement, Hillebrand, Van der Waals and Roelofs and DeSilva report that if necessary movement is improved if the second stimulus is more insistent, while Neuhaus presents good evidence that only the first stimulus is decisive. We have already mentioned Korte's reports of δ-movement under these conditions (cf. under *c, ii*).

(iii) Conventionality. Steinig was the first to contend that the simple lines used as stimuli in most of the experimental studies since Wertheimer, who had set the fashion, were not conducive to the most 'real' and insistent perceived movement. It has been noted by many observers that the simpler and more conventional the stimulus-patterns, the more difficult it is to arouse clear and definite perceptions of movement.

(iv) Similarity. Since Linke, it has been common knowledge that the two stimulus-patterns need not be identical in order to arouse a perception of movement. The limits of difference, however, have not been put to experimental test, and a full description of what occurs under widely unlike stimulus patterns would be of extreme value.

(v) Familiarity. The more nearly the object seen in movement has correspondence to our everyday perceptions of moving objects, the greater will be the possible range of other stimulus variables. This fact is attested to by many; Blug lays special emphasis upon it.

(2) Intra-organic variables (observational conditions)

(a) Observational attitude

(i) Naïve vs. trained Os. This important aspect has received little treatment in the literature. Neuhaus alone has given serious treatment to it, emphasizing the fact that the data he reports are only for trained *O*s, that *O*s naïve in this situation have difficulty in perceiving movement at all under conditions which are optimal for trained *O*s. This problem deserves exhaustive treatment.

(ii) 'Analytic' vs. 'synthetic' attitudes. Wittman, Neuhaus, and DeSilva present evidence that a movement perceived under a passive attitude may be broken down by a searching attitude. The distinction can be forced too far and the

terminology smacks too much of integrative theories, but the fact serves as a useful counterblast to the overemphasis upon stimulus-determinants evinced in the *Gestalt-psychologie*. The question of observational attitude requires further work before definitive statements can be made.

(b) Instruction. As is typical of most experimental studies in psychology, the emphasis here is only on one variety of instruction, the formal type. The possible effects of occasional and self-instruction are hardly, if ever, discussed. DeSilva and (especially) Neuhaus report that a formal instruction to see movement may arouse the perception under conditions which have been hitherto unfavorable. Certain of the configurationists also present evidence that the course and character of the movement may be altered by suitable instruction. From our knowledge of its place in other forms of perceiving, instruction should be an important variable in this setting, and ranks with the other problems demanding immediate investigation.

(c) Attention. Used much more as an explanatory concept than as an experimental variable. Some men, however (Marbe, Linke, Wertheimer, Hillebrand), have used it in the latter as well as the former sense. They instruct their *O*s to 'direct their attention' now here, now there, upon the experimental field. In the sense used, 'direction of the attention' on the field between the two stimulus-positions is more favorable, contrary to Marbe's opinion. Hillebrand reports that fixation upon the first stimulus and attention on the second tends to break down the movement. This result has not been confirmed.

(d) Kinesthetic factors. As long ago as Stern and Dürr and as recently as Higginson, eye-movements have been urged as a necessary condition for the perception of movement. The bulk of the evidence (cf. Linke, Wertheimer, Guilford and Helson) tends to discredit this contention. That eye-movements may, under certain conditions, play a part seems undeniable, but it is also true that 'good movement' may be seen where there is no question at all of the absence of ocular displacement.

(e) Retinal factors. Some spatial change in retinal stimulation is fundamental, although it need not be a continuous change. The number of variables indicated destroys the notion that the perception of movement may be accounted for purely in peripheral terms. It cannot be denied that retinal events are of decisive importance in initiating the perception, but the precise character of the change is more difficult to specify. It seems that practically any change—spatial, temporal, intensive or modal—may, under certain conditions, bring about a perception of movement.

(f) Historical factors. Many who have worked upon this problem insist that the presence of a 'set,' a 'tendency,' or an 'expectation-to-see-movement' may arouse the perception under stimulus-conditions which are markedly unfavorable in the absence of such tuning. The means utilized to arouse such tendencies have varied, some researchers using the effects of instruction, others simply observing the effects of just previous perceiving upon present perceiving. Higginson and Neuhaus are the best references. This has profoundly important implications for the general problem of the development of this performance and merits careful study.

The above outline presents a macroscopic view of the results of two generations of work upon the conditions underlying the 'visual' perception of movement. What general statements can we make about this array of variables? First of all, it may be said with emphasis that it is impossible, at least at present, to reduce the government of this variety of perceiving to a few fundamental laws of stimulus-relationship. The time has not yet come to formulate 'laws,' even if we qualify them by the saving phrase 'all other conditions remaining constant.' Of the twenty or more significant variables which have now received treatment, we

can simply make the general statement that any or all of them, in a large number of possible forms of combination, may suffice to bring about a perception of movement. This situation is comparable to that obtaining in the field of the perception of depth, where a correspondingly large number of conditions have received experimental verification. As experimentation proceeds, the error of over-simplification becomes more and more grave, and dogmatic 'laws' of stimulus-dependency become more and more improbable. This situation should not induce despair. The human organism is so complex and has so long and so varied a history, that it should not surprise us when it performs adequately under what appear to be the most inadequate conditions. As a final statement, we can say that the experimental literature in hand demonstrates conclusively that almost any change in the stimulating agencies, presupposing a properly tuned organism, may arouse a perception of movement.

Where many variables underlying an event have been distinguished and, furthermore, where a given investigator has usually dealt with a small number of these variables, we may well expect a confusion of theories. When we take into consideration the fact that this problem has been approached by men of the most diverse training and interests, the situation becomes even more complex. Many of the theories differ only in details. In others, the differences are more significant than the similarities. Our treatment will be facilitated if we neglect minor differences so far as possible, and turn our discussion into more fundamental, if not quite so well-defined, channels.

In its commonest form, the problem presented by the apprehension of movement under discrete stimulation has been to give an accounting for the apparent discrepancy between the known characteristics of the stimuli and characteristics of the perceived object or event. That such discrepancies exist is an obvious fact in perceiving. For example, a retinal pattern of excitation may be found to be roughly elliptical and broken, yet *O* reports the perception of a perfect circle; it may be flat, yet *O* can report objects as near or far. Auditory stimuli may be presented at equal intervals and with equal emphasis, yet *O* may report an auditory rhythm of a complex pattern. In the field which we have been discussing, the stimulus-situation seems, at first glance, to present one of the most inexplicable of these discrepancies. In the perception of 'apparent movement' the stimulus is *plural* and *stationary* yet, under certain conditions, consistent reports are obtained of a *single, moving* object.

Most of the men who have attempted to give a consistent psychological account of this problem have eased their task of explaining the lack of

parallelism between stimulus and perceived object by an uncritical interpolation of mentalistic intermediaries. In order to bridge the gap, these men have adopted mental mechanisms, differing in complexity and organization from one theorist to another, but all proposed for the same general purpose. First of all, we have the 'sensation,' the building block of mental 'structure,' which seems to possess the virtue of retaining a threatened psychophysical parallelism. For the older theorists, Mach, Exner, and others, it sufficed simply to make a claim for a 'sensation of movement,' a mental entity just as directly bound up with the stimulus as is a sensation of tone or color. As investigation of this problem proceeded, and as psychological theory became more sophisticated, it became clear that such a way out was too easy. The concept of the sensation became progressively more restricted in scope, so that, although there remained controversy upon the question of the particular attributes or qualifications of a sensation, there was general agreement that the term should be limited to such simple, unanalyzable items as tones, colors, pressures, and pains. Seen movement was now classified and treated as a perception, a complex mental event based upon sensations as elements.

This restriction of the doctrine of sensations brought with it a new problem. Means must be provided for bringing the separate sensations together and patching up the possible, and very frequent, gaps between the sensations assumed to be present and the perceived object. Here an active mind was postulated, replete with powers, in order to bring the sensations and perceptions into accord. In whatever fashion this is done, whether by 'fusion of sensation to a unity,' by the "association of ideas,' by 'creative synthesis,' by 'the attention,' by 'dynamic unities,' or by any other of the proposals found in the literature upon perceiving, it still remains true that we are dealing with postulated mental mechanisms, assumed to bring stimulus and product into agreement.

Before we proceed with criticism of this attack upon the problem, it may be well to specify which of the investigators under discussion introduce mentalistic assumptions into their theories. The following table indicates, for each researcher, the concepts utilized.

Investigator	*Mental Existences*	*Mental Activities*
Exner	sensation of movement	Schlussprozesse
Stern	sensations (visual & kinesthetic)	an active mind (Schlussprozesse)
Marbe	sensations (visual)	attention
Linke	sensations (visual)	assimilation
Dürr	sensations (visual & kinesthetic)	apperception
Schumann group	sensations (visual)	attention

Benussi	sensations (visual)	Produktionstätigkeiten
Wittman	Sehdinge	an active mind
Hillebrand	Sehdinge	attention
Van der Waals & Roelofs	Sehdinge	an active mind
Neuhaus	sensations (visual)	none
Dimmick	sensations & images (visual)	none
DeSilva	sensory & perceptual processes (visual)	attention

The assumption of an active mind is one of the most primitive beliefs of mankind. This mind must have 'stuff' upon which to work; there must be mental materials which the mind uses in various ways, just as there are bodily materials employed by the body in its physiological activities. These mental existences are the 'sensations,' 'images' and *Sehdinge*—the mental existences utilized by our theorists. The mind must also be active in various ways; there are agencies and powers peculiar to it, distinct from the powers and agencies peculiar to the body. The agencies and activities are represented, in the theories under discussion, by such terms as 'attention,' 'assimilation,' 'apperception,' and so on. Thus we have mental existences upon the one hand and mental activities upon the other, the 'contents' of mind and the 'acts' of mind.

The most serious charge against these mentalistic theories is that of the *multiplication of psychological entities*. They interpose a series of hypothetical psychological steps between the stimulus and the perceived object. These mental mechanisms seem necessary to the theorists whom we have been discussing simply because of a fundamental misconception from which they have not been able to free themselves. So long as there remains a distinction between mind and body, so long as a psychology is based, explicitly or implicitly, upon a dualistic view of the organism, just so long will an account of psychological performance remain a needlessly complicated and hypothetical affair. Deny these mentalists their basic assumption —which they make upon the basis of the scantiest of evidence—and their whole complicated structure of existences and activities becomes superfluous.

An interesting chapter in the history of this field is devoted to the researches and theories of the configurationists. The work of Wertheimer and his followers affords a useful counterblast to those theories assuming mental intermediaries between stimulus and perception. Rejecting the doctrine of sensations, the adherents of *Gestalt* treat perceiving as a unitary activity, instead of as a construct based upon simpler mental elements. In their own attempt to bridge the gap between stimulus and perceived object, the configurationists offer a physiological theory to take the place of the complicated sensationalism of their predecessors and contemporaries. They have, besides, over-used the notion of unity.

Because of the comparative recency of their views and the contentious nature of their statements, characteristic of a psychological 'school,' it is difficult to estimate the contributions of the configurationists. Their analysis of the stimulus-factors has been fruitful and suggestive, and their rejection of mental existences represents a step forward in the history of this field. At the same time, it must be emphasized that they substitute a premature brain-theory for a more honest and exhaustive account of the governing factors at the basis of perceiving. If it were true that perceiving is governed solely by stimulus, the configurationists might have been justified in turning to theory as the remaining possibility. But there are other variables upon the side of the organism (see our summary, pp. 34-36) which require much more extensive investigation before we can say that our experimental study of this performance is approaching completion. The configurationist's emphasis upon stimulus, in connection with the properties of an hypothetical brain-field, has led them to substitute another 'constancy hypothesis' for the older theory, against which they successfully polemicized during the early years of the school. At basis, their approach to the problems of perceiving is non-historical, in the sense that they make no attempt to account for the development of the performance. The human organism is a growing, developing system, and an adequate account of its activities must include a long-term, as well as a cross-sectional, study.

Were we to list all the investigators who have invoked either mental entities or neural hypotheses in their accounts of the perceiving of movements, we should find the list long. With one or two exceptions, all the men reviewed have used one or the other of these two modes of explanation. Is there any other way out, or are neural theories the only alternative to mentalistic explanations?

We must begin by unequivocally rejecting all mental mechanisms whatsoever interposed between stimulus and product, thereby turning the emphasis away from the structure of a mind to a consideration of the performances of an organism. Furthermore, we do not consider it fruitful to substitute neural hypothesis for a serious investigation of the variables involved in a situation. In accounting for the discrepancies between stimulus and perceived object or event, which served as the guiding posts for older views, we simply state that there are *other* conditions operative than those of the stimulus, that our account of perceiving must consist of a teasing-out and enumeration of these other conditions. This search will remain unprejudiced from the outset if we regard perceiving simply as performance lying within the capabilities of certain organisms and if we

avoid the summary hypostatization of a theoretical mind or an equally theoretical nervous system.

There are really two fundamental problems involved in the investigation of perceiving, as indeed of any psychological performance whatsoever. The first of these problems is to give a suitable account of the conditions (within or without the organism) which must be satisfied *at the moment of perceiving*. The second problem is concerned with the *development* of the performance. The two solutions are of equal importance for a satisfactory account, as we shall try to show. Only the first problem, however, has received extensive treatment in the experimental literature upon movement; the second has hardly been touched. Yet it is precisely the lack of adequate information upon this second aspect, not to mention the almost universal absence of recognition of its existence as a problem, which has determined the directions which theories of movements have taken.

It is the view of the writer that no adequate account of perceiving can be written until more is known of its development during the life-course. The fact that, in the adult human organism, perceptions of depth, form, color, movement, rate, change, and other aspects of objects and occurrences may be called forth by a variety of conditions, and that, moreover, the same extra-organic conditions may lead now to one, now to another variety of perceiving, leads us to suspect that we must look into the past to complete our account. This does not mean that we must assume such ill-defined entities as 'experience' or 'inference' to account for present function. Rather must we point to the obvious fact that earlier performance exerts some influence upon later performance, that psychological operations develop, grow, and change. The adult human organism is adept. It may perceive movements under conditions which appear to be inadequate. Whence comes this ability? How does it develop? The answers to these questions, insofar as experimentation makes these answers available, may help to clarify some of the most puzzling and unknown characteristics of perceiving.

Certain of the problems raised by these questions may have to await the development of more adequate methods. There are certain fundamental difficulties standing in the way of investigation of perceiving at the lower age-levels; these are essentially difficulties of recording and report. In the absence of direct methods we must content ourselves with more indirect procedures. Certain of the techniques evolved in the manifold studies of learning will aid us here. With these techniques we may be able to answer certain questions which bear intimate relationships to the questions of functional development. What is the effect of variation in instruction

upon the apprehension of movement? What differences are there between the performances of completely naïve *O*s and *O*s practiced in observing movement under discrete stimulation? Will the unpracticed *O*s find difficulty in perceiving movement under forms of stimulation which are adequate for trained *O*s? Can we find certain significant changes in the performance as a situation is repeated? What is the place of 'identity' in the problem (a question raised since Linke, but never completely settled)? Does 'identity' operate as a governing factor, as an expression of functional development and 'tuning,' or is it an aspect of the perceiving itself? Answers to these questions should throw light upon problems of development, although the approach is indirect.

The suggestions for research indicated above include problems fundamental to a knowledge of perceiving in general, as well as to that of the perceiving of movements. Experimentation has provided a large store of information upon certain aspects of the governing factors involved in its arousal, information which has stood the test of cleverly devised experimental controls. We know a good deal about the stimulating conditions, and we possess suggestive, if incomplete, data upon the limits of variation of these stimulus-factors. But when we regard perceiving as a function, as a mode of psychological performance, we should attempt to find out something about the performance itself, as well as about the present conditions of its arousal. A study of the history and development of the perceiving of movement is essential before it can be said that our knowledge of this mode of psychological performance is approaching completion.

EYE-MOVEMENTS DURING 'RETINAL RIVALRY'

By ROBERT H. PECKHAM, Temple University

The present study was undertaken because the phenomenon of retinal rivalry, although frequently investigated, has never been subjected to a detailed study of the movements of the eyes during its occurrence. No one knows definitely whether eye-movements are made when the eyes become temporarily anoptic (either completely for their whole pattern, or only partially with respect to some detail) nor whether the movement, if it does occur, is sufficiently large to displace the image into the periphery. If the image is peripherally displaced, the explanation of rivalry becomes a simple matter. If it is not, present speculations regarding the functional processes of the central nervous system obtain an added degree of plausibility. Moreover, eye-movements, even if too small to displace the image from the macula, might induce anopsia. It is obviously important to find out how the eyes behave during these perceptual variations. It can be observed that when a subject traces an outline figure in a stereoscope with a stylus—where the image for one eye is complete and the other partial —that the act of tracing produces partial anopsia at the region of the stylus-point. It is therefore important to find out what happens to the fixation processes at this moment. Also, the relative effects on rivalry when tracing in a pseudoscope instead of a stereoscope should be studied.

Helmholtz listed three stimulus-conditions of retinal rivalry.[1] They are: (a) when the visual fields are different but homogeneous; (b) when only one field contains non-homogeneous elements; and (c) when both fields contain non-homogeneous elements. When such fields are presented stereoscopically, "sometimes there will be alternations, so that, where for a while only parts of one image were visible, presently parts of the other image will emerge and suppress portions of the first image. . . . This fluctuation, in which parts of the two images mutually supplant each other, either side by side, or one after the other, is what is usually meant by rivalry between the visual globes."[2] It is under condition (c) that rivalry is most marked. Helmholtz explained the phenomenon in terms of attention, concluding "(a) that man possesses the faculty of perceiving these images in each eye separately, without being disturbed by those in the other eye, provided it is possible for him . . . to *concentrate his whole attention* on the objects in this one field; . . . (b) the content of each separate field comes to the consciousness without being fused with that of the

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[1] H. Helmholtz, *Physiological Optics*, English trans., 3, 1924, 493 ff.

[2] *Ibid.*, 494.

other field by means of organic mechanism; and that therefore, the fusion of the two fields into one common image, when it does occur, is a psychic act."[3]

Like those of Helmholtz, subsequent investigations of the phenomenon of 'retinal rivalry' have been exclusively subjective, and of the many conclusions drawn and theories proposed, none have been founded upon objective data. Although these descriptive essays are interesting in their delineation of the many aspects of the problem, they are not essential to the present discussion.

Preliminary Experiments

Our purpose in the preliminary experiments was twofold: (a) to delimit the conditions necessary for the appearance of 'retinal rivalry'; and (b) to determine what apparatus would be required in a more exhaustive study.

Apparatus. The apparatus used was an enlarged Brewster stereoscope.[4] In this modification of a familiar apparatus, a pair of plus spheres was set before the field

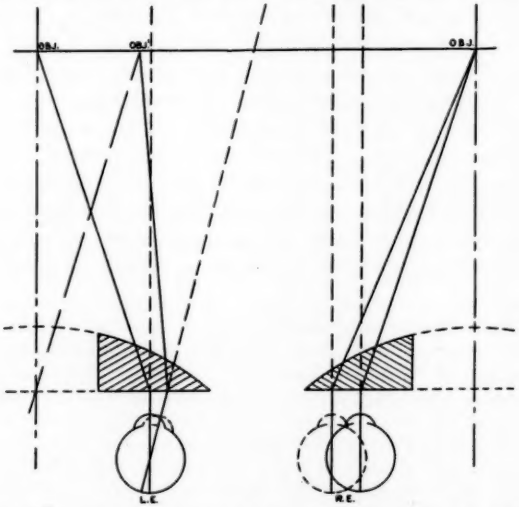


Fig. 1. Brewster Stereoscope as Adapted for Ophthalmic Needs
Obj. = Object Obj.' = Deviated object

of the instrument so that their focal length was equivalent to their distance from the field. Their optical centers were separated by an arbitrary distance of 170 mm. Zero marks were placed on the field with the same separation. Light coming from a zero point will be refracted by the lens to infinity. Further, any beam of light from this point, passing through any given point in the lens, will be likewise refracted,

[3] *Ibid.*, 499.
[4] D. Brewster, Law of visible position in single and binocular vision, etc., *Edinb. Phil. Trans.*, 15, 1844; also, *Phil. Mag.*, 24, 1844, 356-439.

neglecting spherical aberration. Any beam from a source off this point, in the plane of the field, upon refraction will leave the lens with the same angle to the optical axis as the principal ray from this point had before refraction.

Thus an eye, as in Fig. 1, can be placed in any position behind this lens, and yet must be directed straight ahead, parallel to the optical axis, in order that the image fall upon the fovea. Light from a point off the center mark will be so refracted as to require the eye to turn in its direction, through the same angle that its principal ray makes with the axis, in order that the fovea be below its respective image. Conversely, an eye which deviates will require that light, to fall upon its fovea, must come from a point in the field deviating in such a manner as to make its principal ray parallel to the angular deviation of the eye from the optical axis of the lens. The field was marked in rectilinear coördinates of one prism diopter each. The lenses were set in a sliding head-piece providing changes in focus, and the field was mounted upon an adequate support to permit comfort and stability in the use of the instrument.

Procedure. Two yellow cards, one with a round spot 1.3 cm. in diam. and the other with a black square of the same size, were placed before the left and right

FIG. 2. SHOWING "BALL-BAT" DESIGN AND THE TYPICAL RESULT

eyes respectively. The black square was placed on the zero or center mark of the field. With both of his hands in the visual fields, *S* was required to move the red spot so that it appeared to be directly above the black square—"like a red golf ball on a black tee." This same test was repeated for the other hand and eye.

After determining the "phoric" conditions of *S* in this manner,[5] it was next desired to ascertain some condition of rivalry which could be sufficiently controlled. As already stated, "attention to novel elements" in the stimulus-pattern for one eye will tend to bring that eye and its pattern into dominance. When we asked *S* to trace a pattern, which was presented to one eye, on the empty field before the other eye, the blank area surrounding the stylus perceptually dominated the pattern, for a short period of time.

The design used in the preliminary experiments was a crude picture of a ball-bat, with the lines 2 mm. wide (Fig. 2). This contour was chosen because it was simple and easy to talk about, enabling *S* to report his perceptual changes with less confusion. In order to produce both rivalry and fusion, thus at once delimiting the rivalry-producing conditions and maintaining some control over the eye-movements, the lower ¾ of the ball bat was removed on one side. Two cards were used, one imprinted with the complete ball bat, and the other with the uppermost portion

[5] This procedure, called the "phoria test," is a modification of ophthalmic practice. It should be included in any form of stereoscopic investigation, because of its importance in establishing proper convergence.

of the bat only. Rivalry was introduced by requiring *S* to trace, with his stylus, the outline of the bat on the card that contained only a partial contour. The complete picture was set in the zero position, and the incomplete picture at the phoric deviation measured for that eye. *S* was requested to make any necessary change in

TABLE I

RÉSUMÉ OF THE RESULTS OF THE PRELIMINARY EXPERIMENTS

S	Records	Total time (sec.)	No. of disap.	Av. time of disap. (sec.)	Av. no. of disap. per sec.	
					R.H. L.E.	L.H. R.E.
1	R-1	27.2	3	.82	.110	
	R-2	22.8	2	.55	.087	
	R-1, 2	50.0	5	.68	.100	
	L-1	34.1	7	.87		.205
	L-2	28.2	7	.46		.248
	L-1, 2	62.3	14	.66		.225
	R2-1, 4	106.8	14	.80	.131	
	L2-1, 4	116.3	25	1.09		.202
2	R-1, 3	163.1	27	1.47	.165	
	L-1, 3	128.5	20	1.20		.156
3	R-1, 3	69.9	11	1.98	.157	
	L-1, 3	60.5	9	.95		.147
4	R-1, 3	124.9	26	.42	.213	
	L-1, 3	69.2	18	.82		.254
5	R-1, 4	103.8	15	.39	.142	
	L-1, 4	127.9	34	.98		.230
6	R-1, 3	107.6	13	1.91	.120	
	L-1, 4	141.1	28	1.64		.195
7	R-1, 3	52.8	15	1.39	.284	
	L-1, 5	55.6	14	1.94		.250
8	R-1, 3	68.5	13	1.20	.191	
	L-1, 3	48.4	2	.55		.034
9	R-1, 3	62.0	11	1.75	.175	
	L-1, 3	169.5	37	2.08		.209
10	R-1, 3	173.1	23	2.24	.131	
	L-1, 3	128.6	16	1.82		.131
11	R-1, 3	89.3	10	1.32	.104	
	L-1, 3	98.4	14	1.09		.132
12	R-1, 3	117.7	19	2.21	.162	
	L-1, 3	112.9	16	1.75		.139
13	R-1, 3	81.2	21	.87	.218	
	L-1, 3	85.5	23	1.19		.265
Totals	89		491			
Av.		31.3	5.5	1.29	.174	.183

this position for comfortable 'fusion' of the two objects. A simple spring key, similar to a telegraph-key, was supplied to the other hand in a convenient position. *S* was given a few trials in recording rivalry as manifested by the partial or total disappearance of the object pattern. When such a disappearance occurred he was to press down on his key, and stop tracing until the image returned. No attempt was made to record the disappearances of the stylus, since on only four occasions such disappearances were reported. The most common disappearance was that a portion of the pattern would fade away for a few millimeters above and below the

stylus-point, and would stay away for several seconds. Upon return of the object-perception around this small area, the tracing was continued until the next disappearance. No record was taken of the particular part of the ball-bat that disappeared, since no area or section seemed to disappear more often than any other. A typical description of the disappearance is shown in Fig. 2.

The silent contact key was set in series with the pen of a Cambridge three-pen ink-recorder. A metronome, beating half-seconds, controlled another pen. A record of the beginning and end of each cycle of tracing was made by *E* with the third pen.

Results. First, we shall study the records of 'disappearances.' These records demonstrate the existence and frequency of the phenomenon. Then

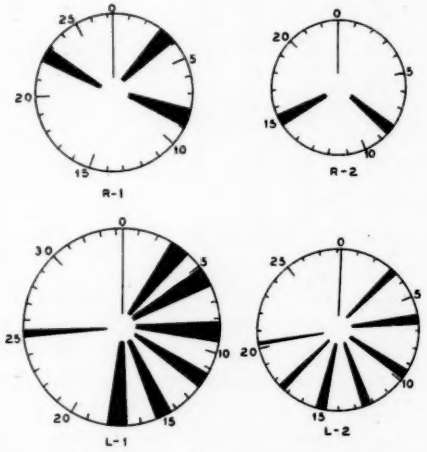


FIG. 3. TEMPORAL SEQUENCE OF DISAPPEARANCES OF *S* 1
Circumference represents total time.

we shall turn our attention to more intimate studies with *S*s 1, 9, and 13. Then we shall be prepared to discuss the apparatus and procedure to be used in a more extensive study. We consider first the results of *S* 1. His preliminary records are presented in detail. The record contains two trials with the right hand and two with the left. *S* was right-handed, as judged from tests in tapping speed, mirror writing and tracing, and general habitual use. Table I gives us the total time for every trial for *S* 1, and the totals of the times for the right and the left trials for the other *S*s. We find, from the italicized items, that *S* 1 reported three disappearances during his first (R-1) tracing, which occupied him for 27.2 sec. The average length of these disappearances was 0.82 sec. During the

TABLE II

THE GROSS RESULTS OF THE EXPERIMENTS WITH THE STEREOSCOPE

Condition	S	Records from the right field					Records from the left field				
		No. trials	Time (sec.)	Disappear.	Eye-move.	Correspond.	No. trials	Time (sec.)	Disappear.	Eye-move.	Correspond.
Tracing & Pattern-Eye	B	3	30	2	6	1	3	47	9	17	6
	C	2	79	8	21	5					
	D	3	61	5	5	1	4	93	4	18	0
	E	3	129	16	44	11					
	F	4	46	5	10	3	4	42	5	16	5
	G	3	141	15	30	11	3	107	13	33	9
	H	3	79	24	21	8	3	87	22	32	15
	I	5	103	21	28	15	7	186	37	45	22
	J	1	18	3	6	1	2	29	9	14	7
	K	2	226	26	50	15	1	149	23	44	16
	M	4	74	14	22	9	3	102	17	27	14
	N	3	50	7	9	6					
	O	3	127	32	25	6	2	103	22	31	17
	P	3	53	8	20	7	3	46	9	16	4
	R	2	77	9	14	6	2	122	14	25	8
	S	2	26	4	10	4	3	36	6	12	1
	T	3	155	16	31	8	2	97	11	19	8
	U	4	50	8	14	4	4	81	8	15	4
	V	3	68	8	27	7	3	77	10	22	8
	W	3	135	20	20	7	2	96	9	19	8
	X	3	129	18	19	6	3	131	39	36	18
	Totals:	62	1856	269	432	141	54	1631	267	441	170
	R+L	116	3487	536	873	311					
Tracing & Tracing-Eye	B	3	32	3	10	1	3	40	7	12	4
	C	2	71	12	9	3					
	D	3	51	3	4	0	2	29	2	3	0
	E	2	76	8	12	5					
	F	2	20	3	5	1	2	19	3	5	1
	G	3	163	23	34	10	3	116	19	31	13
	H	3	76	22	21	14	3	69	22	21	12
	I	4	76	13	14	8	6	109	18	31	13
	J	2	31	7	4	2					
	K	1	72	10	14	5					
	M	2	42	6	4	3	3	78	15	21	3
	N	3	55	8	10	6					
	O	2	73	15	13	6	2	83	14	18	12
	P	2	37	3	9	2	3	50	9	15	7
	R	1	54	9	17	8	2	95	13	14	7
	S	2	20	3	11	2	3	40	11	13	6
	T	3	214	28	37	13	2	72	7	9	2
	U	3	48	7	12	3	1	18	2	4	1
	V	3	71	7	20	6	3	74	11	19	11
	W	3	121	16	3	2	1	29	2	1	0
	X	2	65	13	10	6	3	129	39	26	13
	Totals:	51	1468	219	273	106	42	1050	194	243	105
	R+L	93	2518	413	516	211					

TABLE II (*Continued*)

THE GROSS RESULTS OF THE EXPERIMENTS WITH THE STEREOSCOPE

Conditions	S	Records from the right field					Records from the left field				
		No. trials	Time (sec.)	Disappear.	Eye-move.	Correspond.	No. trials	Time (sec.)	Disappear.	Eye-move.	Correspond.
Without Tracing & Pattern-Eye	C	1	55	13	16	6	1	51	11	13	5
	F	1	45	4	13	4	1	63	5	10	2
	I	1	54	7	4	0	1	70	11	7	3
	K	1	61	6	13	4	1	49	6	11	3
	N	2	65	14	22	9	2	93	14	28	10
	R	1	70	7	10	2	1	41	3	7	1
	S	1	40	3	39	3	1	42	7	28	5
	V	1	25	2	7	1	1	23	4	9	3
	Totals:	9	415	56	124	29	9	432	61	113	32
	R+L	18	847	117	237	61					
Without Tracing & Tracing-Eye	C	1	69	12	19	7	1	55	16	13	6
	F	1	52	3	15	1	1	62	3	13	1
	I	1	70	11	9	5	1	71	9	10	3
	K	1	47	6	9	5	1	75	9	15	2
	N	2	109	29	30	23					
	R	1	46	5	10	2	1	49	6	7	3
	S	1	64	10	72	10	1	32	4	22	3
	V	1	22	3	8	2	1	22	5	9	3
	Totals:	9	541	79	172	55	7	366	52	89	21
	R+L	16	907	131	261	76					

two right hand tracings (R-1, 2) he has reported a total of 5 disappearances in 50 sec. of tracing, or 0.1 disappearances per sec. These disappearances were mediated through the left eye, hence the caption R.H.–L.E. (Right Hand-Left Eye). Fig. 3 shows these relations plotted on a circle. The circumference of the circle represents total time, and the shaded sectors the periods of disappearance. There were a greater number of disappearances when using the left hand to trace than when using the right, and the total time for the left hand was longer. The average disappearance for the right hand was 0.92 and 0.55 sec. for the first two trials, and for the left hand 0.87 and 0.46 sec. The disappearances were more frequent and shorter in duration for the left hand for this *S*.

The original records of all the *S*s are similar to those of *S* 1. It is impossible to find consistent temporal or spacial sequence of the disappearances in the results of any of our *S*s. There is one possible exception. At the joinings of the complete and partial picture, in tracing from the top around, counter-clockwise, there is a faded area which may or may not develop into a disappearance, and which is comparable to the fluctuating gray iridescence spoken of by Helmholtz at the intersection of two wide

black bars in a stereoscope, one vertical and one horizontal.[6] The *S*s were warned to distinguish between this incomplete fading and complete disappearance.

Since there is nothing else of value in the detailed data beyond its distribution, and since this distribution is in itself too irregular for any generalized treatment, the data of this preliminary work have been summarized in Table I. Rather than include each trial the data have been grouped as

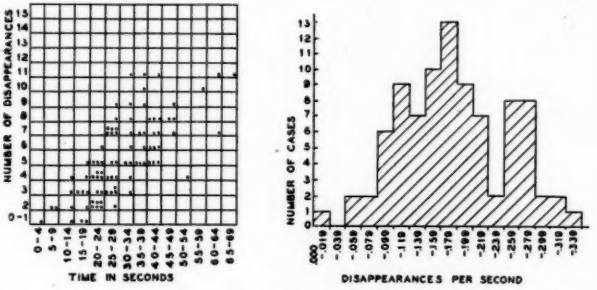


FIG. 4. SHOWING SCATTER AND DISTRIBUTION OF THE RESULTS OF THE PRELIMINARY EXPERIMENT

shown for *S* 1. The total time for the trials, the number of disappearances, the average time per disappearance, and the average number of disappearances are given. It is seen that the disappearances are about as variable in frequency as they are in spacial and temporal sequence. In comparing the even and odd trials, in the original data, this variability was quite marked.

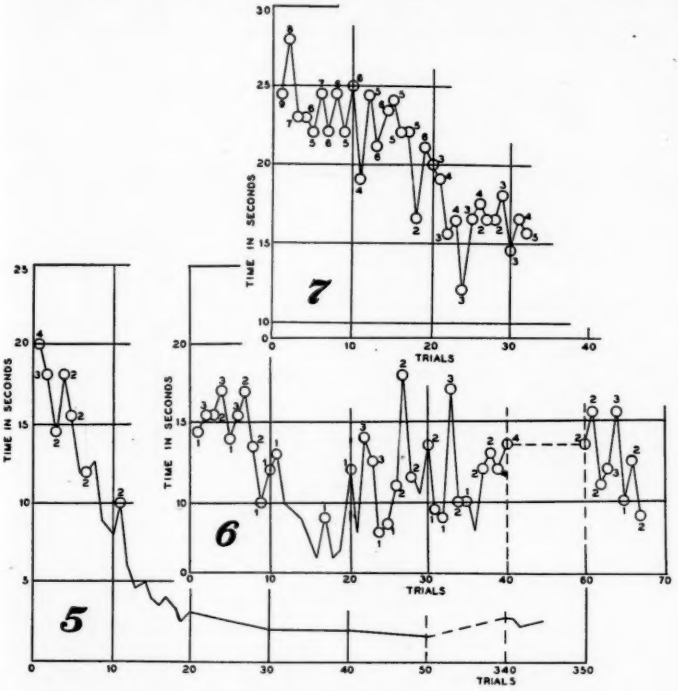
To examine the character of the disappearances, the measures shown in the last two columns of Table II were computed. The time for a tracing and the number of disappearances were to a certain extent mutually variable. *S* had been told to trace slowly, but no definite speed was required. He was, however, told to wait until the disappearances were over before continuing his tracing. Thus more time was consumed when the number of disappearances increased. But when we examine these columns, we find that the number of disappearances per sec. did not follow such a definite proportionality. Hence, the two variables, total time and frequency of disappearances, were not exactly interdependent.

In order to show by some graphical method the interrelations between the two items, Fig. 4 was drawn. The greater the concentration of these items into a line, the nearer a perfect correlation we might expect a

[6] *Op. cit.*, 496.

large group to show. The greater the divergence (scatter) of these items, the lower will be the correlation in a large group.

We find the semblance of a fair positive correlation. We next examine the number of disappearances reduced to units of one second. This score is recognized by the writer to be artificial, it represents only the numerical



FIGS. 5-7. PRACTICE CURVES OF 3 *Ss*
Fig. 5 shows results of *S* 1; Fig. 6, of *S* 9, and Fig. 7, of *S* 13.

quotient of the number of disappearances divided by the time of tracing. It is, however, a definite score of frequency of disappearances from *S* to *S*. If this frequency were regular, within narrow limits, we should conclude that the disappearances are manifestations of a constant tendency in stereoscopic vision—but we do not find this to be the case.

In estimating the differences between right and left hand tracing, from the average number of disappearances for each tracing, we find no reliable

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difference. There is a slight tendency for the *S*s to report more disappearances with the left hand tracing. This may be due to the fact that left hand tracing, being harder, is slower. It is more interesting to examine the individual records. *S*s 1, 4, 5, 6, 9, 11, and 13 show more frequent disappearances when the left hand is tracing. *S*s 2, 3, 7, 8, and 12 show

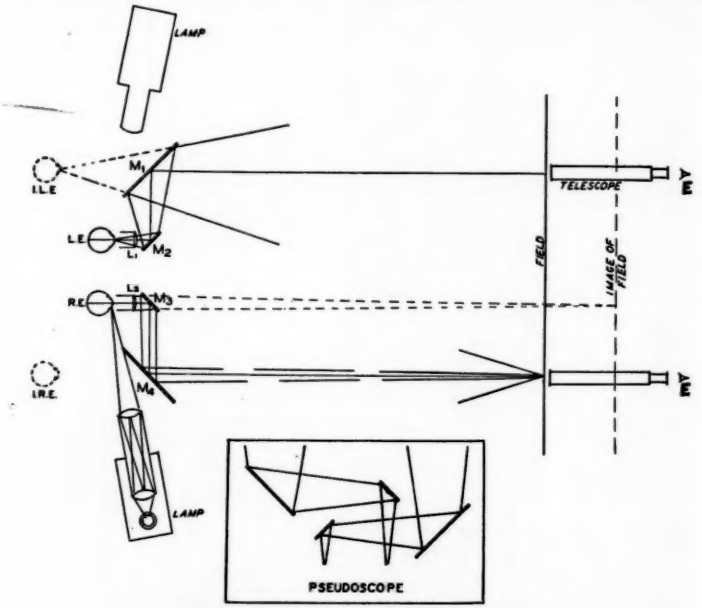

FIG. 8. PLAN OF THE APPARATUS AS VIEWED FROM ABOVE

R.E. = *S*'s right eye
L.E. = *S*'s left eye
L_1, L_2 = Lenses
I.R.E. = Image of *S*'s right eye
I.L.E. = Image of *S*'s left eye
M_1, M_2, M_3, M_4 = Mirrors
E = Experimenter

greater frequency with right hand tracing. All of these *S*s verbally reported right-handedness. We have found, thus far, that the particular stimulus-pattern used could arouse a form of binocular rivalry in 13 *S*s selected only in so far that none were grossly pathologic. We have found that the disappearances were real in the sense that the *S*s could understand instructions concerning them and could record them. We have found that they varied numerically, spacially, and temporally among the *S*s and among the records of any one *S*. We have found certain limits in these

variations. We have, in short, succeeded in delimiting the problem so that more accurate investigation could follow.

Since it appeared immediately that the situation was not constant, it became necessary to investigate the learning possibilities involved. If the phenomenon were only temporary, we could not expect to make any ex-

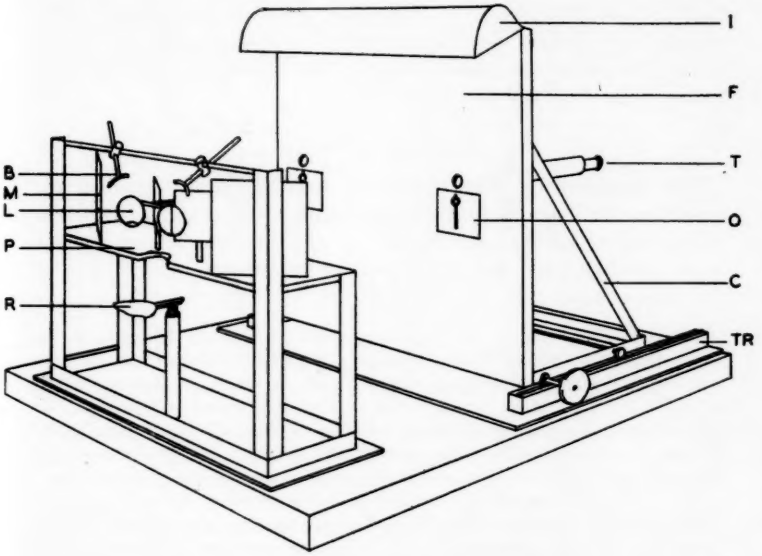

FIG. 9. OUTLINE DRAWING OF THE APPARATUS

B = Brow rest
L = Lens
R = Chin rest
T = Telescope
C = Field carriage
M = Mirror
P = Mirror plate
I = Field illuminant
O = Object
TR = Track for carriage

tensive measurements upon any one *S*. Therefore *S*s 1, 9, and 13 undertook a longer training series. These were continued until it became apparent that there were definite indications of learning, or that no changes were taking place. The three practice curves (Figs. 5, 6, and 7) show the manner of this change. As before noted, the phenomenon is quite individual in character. *S* 1 reported only a few disappearances, and after 11 trials these have virtually disappeared. They appeared again near the end of the record and may have been induced by fatigue. The curve of *S* 9

shows no changes attributable to learning, while *S* 13 shows slight improvement in time and disappearances in 32 trials.

It is evident that subsequent investigation may be safely carried through but a few trials at a given sitting. In the main experiment we have taken our records on different days to avoid training the rivalry away. We are therefore unavoidably exposed to the danger that the causative factors which we are attempting to determine may change in themselves between sessions of recording. This danger was more imaginary than real, since the records showed the frequency of the disappearance varied more from tracing to tracing than from day to day.

Main Experiments

Having determined in the preliminary investigation the conditions and procedure, we sought in the main experiments to determine: (1) whether eye-movements accompanied the rivalry; and (2) whether any relation existed between hand and eye influencing the frequency or character of the disappearances.

Apparatus. Since the Brewster type of stereoscope was not adaptable to the requirements of the present problem, a modified form of Helmholtz's telestereoscope was designed with the following features:[7] optically accurate; adaptable to the pupillary separation of *S;* provision for observing eye-movements; provision for illumination of *S*'s eyes; constant illumination of the field; adequate support of the *S*'s head; proper supporting table; a set of silent spring contact keys for obtaining polygraph records; and a means for changing the stereoscope into a pseudoscope.[8]

The instrument incorporated a method of directly observing eye-behavior by the use of laboratory telescopes. They were directed upon the iris markings near the limbus of *S*'s eyes. To illuminate the eyes, a beam of condensed light was projected tangentially on the cornea. In this manner the iris was illuminated on the outside directly, and upon the inner (nasal) side by virtue of the refractive power of the cornea. The minimum of light entered the eye directly while *S* fixated the test-objects. The illumination of the field was accomplished by two 25-watt tungsten filament tubular bulbs in a reflector. To convert the stereoscope into a pseudoscope, the mirrors, on their posts, were changed to other positions. The mirror-posts were held in place, in either position, by removable taper pins.

The telescopes had a magnifying power of seven diameters. The separation of the objects at the field was 200 mm. The optical distance from the field to *S*'s eyes was 50 cms. The maximum error of the stereoscope (due to undetected variations in the position of *S*'s head, and to maladjustment of the optical parts) was at worst 0.25 mm. at the field, which amounts to an angular error of 17′ 5″. The

[7] *Op. cit.*, 311.

[8] All of these requirements were accomplished with the help of Dr. W. C. Beasley, who also suggested mounting the instrument upon soapstone, since this would prevent loss of accuracy through warping of the metal parts.

least measurable eye-movement that was not ambiguous to *E* was about 0.1 mm. at the cornea. (This is equivalent to about ½° of ocular rotation.)

A four-pen polygraph was used, driven by a shunt-wound motor. The time line was controlled by a seconds pendulum. One pen was used by *E* to record observed movements, one by *S* to record disappearances, and one by *E* to indicate the terminations of each record. Figs. 8 and 9 illustrate the completed stereoscope.

Procedure. In the investigation we wished to determine: (1) whether eye-movements accompanied disappearances; (2) if so, whether they preceded or followed the disappearance; and (3) whether there was any correlation between handedness and disappearances. Testing methods for the determination of these relations had to be developed.

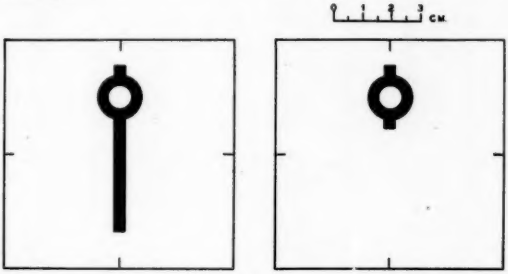

FIG. 10. SHOWING OBJECT THAT WAS TRACED IN MAIN EXPERIMENT

A phoria test was made in the stereoscope and in the pseudoscope. The same test objects and procedure as in the preliminary experiments were repeated.

S was then presented with the test objects shown in Fig. 10. These consisted of a black ring 1.5 cm. outside diam. and 0.8 cm. inside diam. with a vertical bar extending 4 mm. upward from the outer edge and 40 mm. downward from the lower edge. One of these objects was purposely incomplete, the lower bar extending only 4 mm. These pictures were placed in the phoric position, *S* moving them about until they were 'fused.'

He was then instructed to trace the figure with the blackened point of a stylus. He did this tracing on the side of the incomplete figure. After one trial, he was asked to report verbally what had occurred. The tracing was repeated, until he understood what was meant by *E*'s description of "part of the tail fading out, near the pencil." He was then asked to place his free hand on the recording key, and press, then release, the key when part or all of the tail disappeared. These instructions were given verbally, with help and demonstration on the part of *E*.

This study is not, however, open to the criticism of suggesting disappearances. The disappearances were reported spontaneously by *S*, and it was not until he had reported them that the final procedure was demonstrated.

While *S* was tracing, *E* was observing eye-movements through the telescope. and 'Pattern-eye' in the subsequent data. The records for every *S* included six for each hand in each situation. Of these, three of the first included movements of the right eye and three of the left eye. The time required for the first session was about one hour.

TABLE III

THE GROSS RESULTS OF THE EXPERIMENTS WITH THE PSEUDOSCOPE

Condition	S	Records from the right field					Records from the left field				
		No. trials	Time (sec.)	Disap-pear.	Eye-move.	Corre-spond.	No. trials	Time (sec.)	Disap-pear.	Eye-move.	Corre-spond
Tracing & Pattern-Eye	C	1	31	5	9	4	1	30	6	9	4
	F	2	17	3	5	1	2	31	5	13	2
	G	2	27	6	8	0	2	33	9	16	6
	H	2	54	5	11	4	2	55	10	6	2
	I	2	18	2	2	0	2	17	2	4	1
	M	2	61	9	22	7	2	67	10	20	7
	N	1	9	0	4	0	2	22	3	5	1
	O	2	75	11	17	5	2	59	10	19	6
	R	2	91	15	11	2	2	101	14	7	4
	S	2	31	9	9	2	2	17	1	9	0
	T	2	48	4	2	0	2	91	8	9	2
	U	2	53	5	16	4	2	67	8	27	5
	V	2	55	8	18	4	2	47	10	20	8
	W	2	37	6	4	1	2	37	4	4	1
	X	2	110	20	24	10	2	97	17	25	5
	Totals:	28	717	108	162	44	29	771	117	193	54
	R+L	57	1488	225	355	98					
Tracing & Tracing-Eye	C	1	27	5	6	2	1	38	9	12	3
	F	2	17	2	5	1	2	20	2	5	1
	G	2	21	6	5	1	2	29	7	9	5
	H	2	40	8	8	3	2	60	9	6	2
	I	2	17	3	2	1	2	18	3	1	1
	M	2	85	8	28	7	2	58	8	24	4
	N	1	9	2	2	1	1	22	3	9	2
	O	2	82	10	30	6	2	97	14	40	6
	R	2	58	12	7	4	2	88	14	7	3
	S	2	29	4	8	3	2	22	0	11	0
	T	2	26	1	0	0	2	46	3	3	1
	U	2	71	6	21	5	2	72	5	29	2
	V	2	50	7	16	5	2	55	12	15	5
	W	2	38	7	5	3	2	77	10	9	3
	X	2	106	27	29	20	2	86	18	30	9
	Totals:	28	676	108	172	62	28	788	117	210	47
	R+L	56	1464	225	382	109					
Without Tracing & Pattern-Eye	C	1	42	8	6	3	1	30	7	8	2
	F	1	42	3	15	2	1	39	3	15	2
	G	1	51	4	16	3	1	56	8	17	6
	H	1	57	5	13	0	1	56	15	20	8
	I	1	48	2	8	1	1	40	9	9	4
	M	1	48	9	23	6	1	67	7	45	4
	N	1	11	0	2	0	1	57	6	22	5
	O	1	51	8	12	7	1	50	8	16	5
	R	1	56	3	9	1	1	48	3	2	0
	S	1	49	2	21	2	1	49	9	43	9
	T	1	43	5	1	1	1	36	3	4	1
	U	1	47	3	13	2	1	52	5	14	2
	V	1	31	3	11	1	1	24	6	8	5
	W	1	31	5	8	2	1	24	6	8	5
	X	1	44	11	11	7	1	25	6	15	2
	Totals:	15	651	71	169	38	15	653	101	246	60
	R+L	30	1304	172	415	98					

TABLE III (*Continued*)

THE GROSS RESULTS OF THE EXPERIMENTS WITH THE PSEUDOSCOPE

Conditions	S	Records from the right field					Records from the left field				
		No. trials	Time (sec.)	Disap-pear.	Eye-move.	Corre-spond.	No. trials	Time (sec.)	Disap-pear.	Eye-move.	Corre-spond.
Without Tracing & Tracing-Eye	C	1	39	6	7	2	1	26	6	9	0
	F	1	64	4	29	2	1	30	3	9	2
	G	1	51	5	16	4	1	40	6	13	3
	H	1	45	4	7	0	1	65	15	12	6
	I	1	50	5	14	3	1	29	7	8	4
	M	1	54	8	32	6	1	40	4	19	4
	N	1	17	1	12	1	1	56	7	7	2
	O	1	47	7	11	5	1	45	6	18	5
	R	1	63	7	5	1	1	53	6	7	1
	S	1	66	3	25	3	1	50	6	41	6
	T	1	47	3	2	0	1	25	2	1	0
	U	1	37	2	12	1	1	55	6	19	3
	V	1	46	2	18	0	1	19	6	7	3
	W	1	47	9	11	3	1	36	8	7	3
	X	1	51	13	13	7	1	38	7	13	3
	Totals:	15	677	79	223	38	15	607	95	190	45
	R+L	30	1284	174	413	83					

Vertical movements were not recorded, since these were made in following the tracing. All horizontal movements were recorded.[9]

Records were taken with both hands, both eyes, and both apparatuses—the stereoscope and pseudoscope. Since one eye was watching the tracing hand, and the other the vertical bar of the complete pattern, these eyes are labeled 'Tracing-'

A second series added a control evident after the analysis of the first records. For this reason many of the *S*s' records are not complete. The procedure of the first series was repeated without tracing, to check against the criticism that the disappearances were not related to what is usually meant by 'retinal rivalry' but were produced by the act of tracing. This second session consumed about ½ hr.

The third session was the investigation of the disappearances with the pseudoscope. In this case the tracing hand was on the opposite side from the tracing eye, and disappearances of the pattern were mediated through the eye on the same side as the tracing hand. The time necessary was about one hour.

Treatment of data. It was impossible to obtain all of the *S*s for all 3 sittings. There was a total of 574 tests made upon the 24 *S*s. The *S*s' records yielded frequency of disappearance and *E*'s records yielded frequency of eye-movement. These data are shown in Table II for the stereoscope and in Table III for the pseudoscope. As a statistical measure of the reliability, we have computed the coefficient of correlation of the frequency of disappearance in the odd versus the even trials, yielding $+0.60 \pm .06$.

In considering the scattergram of Fig. 11, we are interested in knowing if any sort of numerical correspondence existed between movements and disappearances. From the form of the scatter, apparently they varied in direct proportion. Since this

[9] The horizontal movements were quick deviations from side to side. Most were convergent with divergent return to fixation. No accurate estimate was attempted of the size of the movements. All amounted to ½° or more of rotation.

does indicate what may be a positive correlation had more subjects been used, the writer constructed the frequency histograms of Fig. 12. Since the two curves are so much alike, we are tempted to conclude that a causal relationship exists between fixation (as shown by movements) and 'retinal rivalry,' as shown by disappearances. To test this hypothesis we must know what temporal relationships exist in the data.

We must know whether the movements precede the disappearances, or the dis-

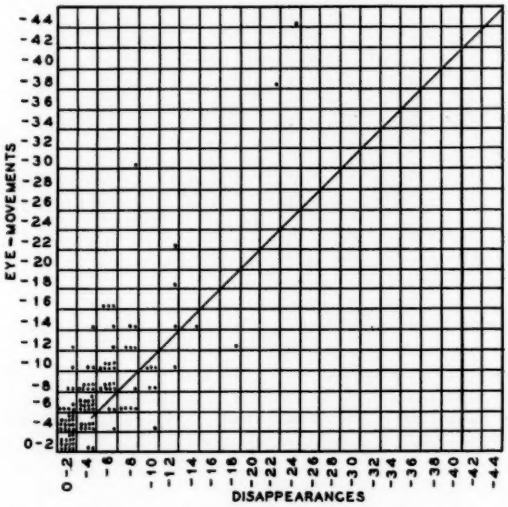

FIG. 11. SCATTERGRAM SHOWING THE RELATION BETWEEN EYE-MOVEMENTS AND DISAPPEARANCES

appearances the movements. We have, therefore, gathered the movements and disappearances which corresponded within an arbitrary time limit. The limit chosen was one second either way. If a movement preceded or followed a disappearance by one second or less, the two are chosen as a corresponding pair. By varying the time limit the degree of temporal causation could be determined, provided such a gathering of items showed positive correspondence beyond that of chance with two independent variables.

One second was chosen as the limit because a longer time (*e.g.* 1.5 sec.) found many disappearances corresponding to one movement. This means that the temporal causation, if it existed, was within a smaller time limit. An attempt to use a smaller time limit (0.5 sec.) showed very little correspondence at all. If there were temporal relations between the two, that relation was not existent in as short a period. An attempt to establish the relation by determining the time between each disappearance and the following (or preceding) movement was obviously a failure, so diverse were the intervals between a given disappearance and the next movement.

Results. There were 1993 disappearances reported in the three series. 3452 records of eye-movements were made by *E* during these disappearances. Of these, 1047 pairs corresponded (*i.e.* ± 1 sec). In order to better visualize and discuss the nature of this correspondence, the records have been summated into groups and these are graphically illustrated. In studying them, the reader is warned to avoid confusing the definitions

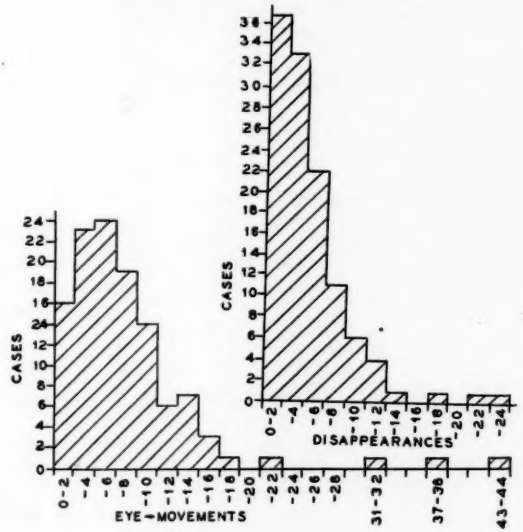

FIG. 12. HISTOGRAMS OF THE EYE-MOVEMENTS AND DISAPPEARANCES

of 'pattern-' and 'tracing-eye' already given. During the experiments where no actual tracing was being done (*i.e.* Series 2), the same system has been applied, and 'tracing-eye' refers to the eye that would have done the tracing.

In the tabulations of Tables II and III, we find listed the cases in which corresponding pairs were found within 1 sec. in either direction. If an exact correspondence existed, there would be no difference between the number of disappearances and the number of movements. Since such a difference is demonstrable, Fig. 13 is included to illustrate this divergence. By using lines whose length is proportional to the total number of movements or disappearances observed, and by representing the corresponding pairs by the overlapping of the lines, it can be shown that there was a

mutual distribution similar to that due to the chance interference of two independent variables. The differences in the total lengths of these figures lie in the number of records of the various types taken rather than to any peculiarities inherent to the situation.

We found in every case that there were as many or more movements that did not correspond to disappearances as there were that did, and in

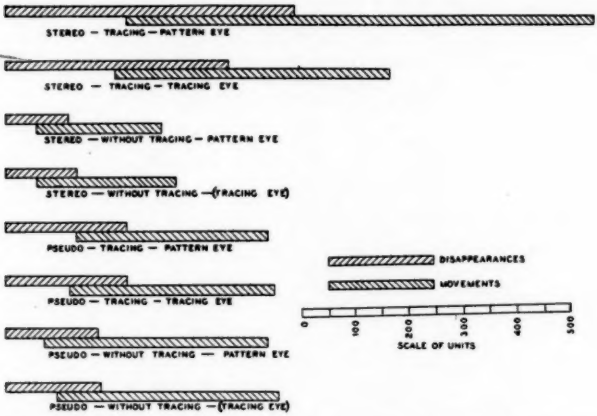

FIG. 13. GRAPHIC ILLUSTRATION OF THE CORRELATION IN EVERY EXPERIMENTAL CONDITION BETWEEN THE DISAPPEARANCES AND EYE-MOVEMENTS

most cases that over half the disappearances were without corresponding movements. To insist that eye-movements and disappearances correspond, is to contend that well over half the movements and half the disappearances were missed in our records. Considering the care involved in the instrumentation and investigation, the writer does not feel that such a contention can be justified.

It might be, however, that eye-movements occurred twice for every disappearance: once at the beginning and once at the end. Were this the case, half the records of movements would have no meaning, since the termination of the disappearance was not recorded by *S*. It would mean that half the records of movements would be temporally related to the records of disappearances, and that there would be approximately twice as many movements as disappearances recorded. There was no such regularity indicated in the records as would be evident if the movements and the beginnings of the disappearances were temporally related in this fashion.

Further, in 8 out of 9 situations, more than twice as many eye-movements were observed as disappearances reported. This relation obtains even if the movements of the 'pattern eye' alone are considered.

If the movements bore a causal relationship to the disappearances, there would be no movements in the 'tracing eye' (save the vertical following movements in both eyes). Since we do find such movements and since neither set, for the 'pattern-' nor for the 'tracing-eyes,' shows a temporal

TABLE IV

FREQUENCY OF DISAPPEARANCES PER 100 SEC.

Apparatus	Eye	Tracing	Without tracing
Stereoscope	pattern	15.3	13.8
	tracing	15.4	15.5
Pseudoscope	pattern	15.5	13.2
	tracing	15.5	13.2

relation for the movements, it follows that the disappearances were independent of the movements. We must determine what other correlations can be found in the data. Were there any effects that might be ascribed: (1) to the act of tracing; (2) to the retinal image of the tracing hand and stylus; or (3) to the difference between the two retinal images when there was no tracing?

The data must be reduced to comparative units, and therefore the fre-

TABLE V

FREQUENCY OF MOVEMENTS PER 100 SEC.

Apparatus	Eye	Tracing	Without tracing
Stereoscope	pattern	24.8	28.0
	tracing	19.2	30.9
Pseudoscope	pattern	24.5	30.3
	tracing	26.7	30.3

quency of disappearances or eye-movements per 100 sec. has been computed. Because of the divergencies in individual differences, we have preferred to average the results and to emphasize the qualitative differences.

Table IV shows the consistent tendency for the frequency of disappearances to maintain constant level during tracing, and for this level to decrease when there was no tracing. The fact that the act of tracing has increased the frequency of disappearance is important. The effect of tracing in the stereoscope has been to increase the rate of retinal rivalry.

We find an anomaly in one case in which the rivalry has not been decreased although there was no tracing. Since there was no effective stimu-

lus change to *S* when *E* was watching either pattern or tracing eye, and since the same condition does not repeat itself in the (effectively identical) pseudoscopic situation, the anomaly remains explainable only in the fact that the sample was so small.

In the same manner the differences in the frequency of movements are demonstrated in Table V. We observe that the frequency of eye-movements was greater without tracing, although without the act of tracing, the frequency of disappearances was smaller. The effect of tracing is therefore to decrease the eye-movements, and to decrease them more on the side of the hand which is tracing. In the stereoscope there were fewer eye-movements per 100 sec. in the tracing eye—the eye on the same side as the tracing hand. In the pseudoscope the reduction is greater in the pattern eye—again the eye on the same side as the tracing hand.

There are two effective stimulus-patterns involved in the tracing, these are: (1) the kinaesthetic pattern due to the movement of the hand; and (2) the visual pattern of this hand moving in the field of view. In the stereoscope these two response-patterns were on the same side, and in the pseudoscope on the opposite side. We find both of these were effective in reducing eye-movements. We find them most effective when they were on the same side of the body. When on opposite sides, we find the kinaesthetic pattern was more effective than the visual pattern.

If attention is defined in terms of vividness or as the proportion of sensory clearness depending upon some particular stimulus-pattern, in this case the field of view in which the tracing occurs, we may say that the act of tracing arouses more attention to this field, increasing rivalry (thus itself an aspect of attention) and decreasing the eye-movements, resulting in increased visual efficiency during the period considered.

From the point of view of 'gradients' we might conjecture that the visual and kinaesthetic gradients when united are effective in reducing eye-movements, but when paired against each other, the kinaesthetic gradient is dominant or more effective than the visual gradient.

SUMMARY

(1) A form of retinal rivalry that can be examined spacially and temporally was defined. This consisted of a vertical bar on one pair of pictures to be used in a stereoscope, the other picture being blank on the corresponding area. To maintain binocular fusion identical parts were included in both pictures.

(2) The rivalry that became manifest with this design took the form of disappearance of a portion of the vertical bar.

(3) When the image of the bar was traced with a stylus on the blank side the rivalry became more pronounced.

(4) In preliminary experiments it was found that with practice the rivalry would disappear.

(5) For the main experiments a large stereoscope of the double mirror type was built. This could be changed at will into a double mirror pseudoscope. Telescopes were incorporated for observing eye-movements.

(6) Using the large stereoscope, simultaneous records of frequencies of disappearance and eye-movements were made.

(7) No relations, causal or temporal, were found between eye-movements and rivalry as manifested by disappearances.

(8) The act of tracing was found to (a) increase the rivalry; and (b) decrease the frequency of eye-movements.

(9) The kinaesthetic pattern involved in the moving of the hand during the tracing was found to be more effective than the visual pattern of the hand moving in reducing eye-movements. This was demonstrated by the difference between the stereoscopic and the pseudoscopic results.

PSYCHOLOGICAL GOVERNMENT AND THE HIGH VARIABILITY OF SCHIZOPHRENIC PATIENTS

By J. McV. HUNT, Worcester State Hospital

Specific pathological change and structural damage have not been demonstrable in that group of disorders called schizophrenia. We must look elsewhere, then, for typifying signs or conditions. To the present writer, this kind of psychosis appears to involve a break-down in government of the psychological functions. He accordingly looks upon observed changes in behavior of the schizophrenic as due to shifts in the weighting of various governing moments.[1] If it is true that a primary defect in these disorders lies in government, then it should be possible to demonstrate by experiment the seat of the defect. The purpose of this paper is to describe an experiment in which certain reportable governors are related to the well-known variations in the output of schizophrenic patients.

High variability has been one of the most consistently demonstrated characteristics of the performance of schizophrenics. It commonly appears when a patient is required to repeat a performance a number of times or from time to time. This irregularity has several times been reported in studies using the methods of test.[2] For example, the standard deviation of measures of a schizophrenic's reaction-time is larger than that of the well person.[3] Schizophrenic patients have also been shown to have a greater than normal variation for the perceived rates of fluctuation in an ambiguous figure.[4] The type of variation which will concern us in this

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[1] For a description of the concept of government, see M. Bentley, *The New Field of Psychology*, 1934, 23-30, 89-90, 173-174, 257-265, 285-287; *The Problem of Mental Disorder*, 1934, 284-289, 304-306; Mind, body, and soul in medical psychology, this JOURNAL, 45, 1933, 577-591.

[2] For descriptions of work with tests among schizophrenics, see S. L. Pressey, Distinctive features in psychological test measurements made upon dementia praecox and chronic alcoholic patients, *J. Abn. Psychol.*, 12, 1917-18, 130-139; S. L. Pressey and L. W. Cole, Irregularity in a psychological examination as a measure of mental deterioration, *ibid.*, 13, 1918-19, 285-294; and J. N. Curtis, Point scale examinations on the high-grade feebleminded and the insane, *idem*, 77-118.

[3] E. W. Scripture, Reaction-time in nervous and mental diseases, *J. Ment. Sci.*, 62, 1916, 698-719; E. B. Saunders and S. Isaacs, Tests of reaction-time and motor inhibition in the psychoses, *Amer. J. Psychiat.*, 9, 1929, 79-112; and F. L. Wells and C. M. Kelley, The simple reaction in psychoses, *ibid.*, 2, 1922, 53-59.

[4] J. McV. Hunt and J. P. Guilford, Fluctuation of an ambiguous figure in manic depressive and dementia praecox patients, *J. Abn. & Soc. Psychol.*, 27, 1933, 443-452.

study, however, has best been demonstrated by Gatewood, who, by obtaining performances on the same tasks successively through several weeks, found schizophrenic patients halving or trebling their output in successive periods of work.[5]

Although wide variation here appears to be a fundamental characteristic of activity, investigators have not attempted to describe its basis. This has been our experimental task. In its solution we have seemed to find that the high variability has its root in factors which govern functional performance and so determine the extent of functional output. Starting from output we have worked backward, by the use of the patient's report and by our experimental controls, toward the disturbing source.

The procedure employed was a combination of the method of report with the measuring of output on a set of tasks performed at successive intervals. The tasks were (1) adding, (2) dealing cards, (3) sorting cards, and (4) substituting numbers for letters in a page of print. The number of pairs of digits added in 2 min., the time required to deal and to sort a deck of cards, and the number of letters viewed in 2 min. constituted the measures of output.[6] The reports concerned (1) prevailing mood on the day of work, (2) the preparation for performance, (3) the course of activity during the execution of the task, and (4) the extraneous occupations indulged in while the task was under way.

A work-period began with a recorded interview concerning *S*'s mood or spirits. The next preceding record of *S* for adding was then marked on a mimeographed page of addition problems and he was asked whether he could beat it. At this point he was carefully questioned regarding the nature of his attitude. Then *E* said, "ready—go," with about 7 sec. between the two words. The performance ran 2 min. *E* then asked *S* to report what he 'had been thinking about' during that fore-period. The nature of *S*'s preparation for performance was gleaned from these two reports. *S* was then asked to describe the course of his performance, his first attempts being amplified through questioning. Finally, *S* was asked whether he 'had thought about anything else' while he was doing the task. If so, he was urged to describe or relate the extraneous occupations. The same procedure was followed with the dealing of cards.

For card-sorting and substitution the directions were altered from day to day. The instruction for card-sorting: "Put those cards above 5 on the right; put those below 6 on the left; and put the face cards in the middle" was alternated with "Put the odd cards on the left; the even cards on the right; and the aces and face cards in the middle." Right and left were also alternated on successive days. This technique was employed to obtain a situation in which *S* had to personalize the formal instruction in order to proceed with the task. The material for the substitution-task was so designed that several pairs of letters occurred in the same frequency throughout the page. A new pair of letters and a new pair of digits were given on each successive day of work. The procedure relating to the reports was practically the same for each task.

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[5] L. C. Gatewood, An experimental study of dementia praecox, *Psychol. Monog.*, 11, 1909, (no. 45), 1-71.

[6] Errors were systematically subtracted from the raw scores.

The disordered *Ss* were 8 schizophrenic patients and one patient (*Si*) whose diagnosis was changed from schizophrenia to psychasthenia during the experiment. None had been hospitalized for a long time, and none could be considered grossly disordered. Only *We* exhibited hallucinations. The symptoms present in the others were confusion, delusion, or the hebephrenic laugh. Of the 9 *Ss*, 2 (*Sz* and *Ro*) were men. All but 2 (*Fr* and *Sz*) had at least finished high school, and 4 (*Re*, *Ka*, *We*, and *La*) had had some college training. One (*We*) had been a lawyer. The ages were 18–30 years.

TABLE I

COEFFICIENTS OF VARIABILITY

	Adding	Dealing	Sorting	Substitution
Controls				
Eh	27	48	81	46
Mb	45	93	211	203
Ms	20	42	101	147
Pe	47	35	55	82
Wb	13	23	95	135
Wo	13	52	101	115
Ta	24	31	54	69
Feebleminded				
Yo	120	35	107	99
Schizophrenics				
Fr	58	161	332	176
Go	127	154	219	272
Ka	151	104	726	240
La	82	141	175	129
Re	88	54	126	130
Ro	101	60	128	148
Si	58	97	159	131
Sz	71	19	34	133
We	71	27	256	237
Paretics				
Do	46	61	140	133
Gr	76	14	198	63
Se	49	15	126	131
Sm	74	66	291	156
Wh	36	69	108	132
Group averages				
Controls	29	46.5	99	112.5
Schizophrenics	89.7	90.8	228.3	177.3
Paretics	56	45	172.6	123

As we were primarily interested in information available only from the report, the patients had necessarily to be accessible. That is a limitation imposed by the method. Especial care was taken to get and hold the patient's confidence, and with all but one (*Ka*), *E* was successful. Not only did the *Ss* appear to enjoy the experimental periods; they came with questions concerning some of their difficulties. The latter, if important, were related to the psychiatrist in charge.

The controls were 4 nurses (*Ms, Pe, Wb,* and *Wo*), a graduate student (*Mb*), a patient suffering from muscular distrophy, but without mental disorder (*Ta*), the writer's wife (*Eb*), and a feebleminded girl (*Yo*). In order to compare two widely differing types of disorder, 5 paretic patients were also included, 4 men and 1 girl who were undergoing or had recently completed courses of radiothermy.

The number of work-periods was set at 15, to be spaced at intervals of approxi-

mately 5 days. Practical considerations, however, made it impossible to obtain 15 samples of performance from every *S*. With three exceptions (*Fr*–11, *Ka*–7, and *We*–7) the schizophrenic patients worked the full number. Of the controls, the nurses worked only 10 times at 2-day intervals, the paretics from 10 to 15 times.

RESULTS

(1) Variability in output. Measures of output were obtained by us to confirm the variability found by other investigators. In order to compare the variability of the schizophrenics' performances with that of the paretics and the controls, coefficients of variability were computed from every *S*'s scores (Table I). The coefficients were obtained by means of the formula, $V = [1000\ (\Sigma\ \Delta - I)]/[\bar{X}\ (n - 1)]$ devised to eliminate the improvement occurring through repetition. Δ is the difference

TABLE II

PERCENTAGE OF DAYS FOR EACH MOOD

Controls	H	N	L	Schizophrenics	H	N	L	Paretics	H	N	L
Eh	13	74	13	*Fr*	0	75	25	*Do*	20	50	30
Mb	13	54	33	*Go*	25	60	15	*Gr*	0	90	10
Ms	10	70	20	*Ka*	0	0	100	*Se*	0	80	20
Pe	10	70	20	*La*	6	40	54	*Sm*	0	80	20
Ta	0	100	0	*Re*	26	35	45	*Wh*	0	70	30
Wb	20	60	20	*Ro*	6	66	28				
Wo	0	80	20	*Si*	20	47	33				
Yo	0	75	25	*Sz*	0	100	0				
				We	14	72	14				
Mean	8	73	19	Mean	12	49	39	Mean	4	74	22

between the output of each pair of successive performances. *I* is the amount of improvement as determined by subtracting the average score of the first two performances from the average score of the last two. $\bar{X}$ is *S*'s mean output, and $(n - 1)$ the number of differences obtainable.

The coefficients make it obvious that, as a group, these schizophrenics are more variable than either the controls or the paretics. A few exceptions, however, should be noted for future consideration. The graduate student (*Mb*) is more variable than any of the paretics, more variable than some of the schizophrenics. One schizophrenic (*Sz*), on the other hand, is one of the least variable, and coefficients of another (*La*) are little larger than those of the controls. Excepting their performances on the card-sorting task, the paretic patients vary little more than do the normal *S*s. For these paretics the alternated card-sorting tasks were not of equal difficulty. It was harder for them to understand "above 5 and below 6" than "odd and even."

(2) Survey of the factors considered in the reports. (a) Mood. To show the sweep of the results, the reports of each factor were classified according to categories derived inductively.[7] There were three moods: (1) high spirits (H), (2)

[7] The writer's wife kept the quantitative records while he classified the reports.

neutral (N), and (3) low spirits (L). Since one perceives only one's own state of being, one has no other standard of reference. *S* can report whether he is above, below, or about average. Finer distinctions proved meaningless for this investigation. For purposes of summary comparison, the percentage of days on which *S* reported each of the three mentioned levels (H, N, L) was computed. The results appear in Table II.

The percentages in Table II show no consistent tendency for these three groups of *S*s to differ in mood. Excepting three of the schizophrenics (*Ka, La,* and *Re*), who were depressed much of the time, the schizophrenics report the same proportion of high and neutral spirits as do the normals. The paretics reported very little elation.

(b) Preparation for performance. Since medical observers have long considered a large proportion of the schizophrenic's difficulties as belonging in the realm of volition, it was expected that some of the more illuminating results could be gleaned from the reports of the patient's preparation for performance. We were especially anxious to get a description of the self-instructions under which the schizophrenic operates and to compare them with those of normal persons and of paretics. Eight categories of preparation emerged from the reports of the fore-period and of attitude toward the task. Each has been labelled with a letter which will appear in Table III.

(1) 'C'=competitive preparation. *S* is confident that he can beat his record. He is mildly excited, and during the fore-period he instructs himself to avoid the pitfalls of previous performances. Here are reports.

Wb (Control) I'm all set to beat it today, Doctor. (What did you think about between the 'ready' and the 'go?') I was getting these sleeves out of the way . . . getting those numbers and letters fixed in my mind. The 6 is like the *b* and I thought that ought to be easy. . . .

Re (Schizophrenic) Maybe I'll beat it again today. (between 'ready' and 'go') "I did something new today; I said to myself '6-7-8-9-10' and pushed my right arm . . . '5-4-3-2-ace,' and pushed my left arm. Then I got just like I used to when I was going to take a test in school. . . .

Se (Paretic) I can beat it. (between 'ready' and 'go') "I was just thinking of the numbers, just practicing a little bit. . . .

(2) 'P'=planned performance. *S* plans his performance with the implicit self-instruction to do his best, but there is no excitement, no reference to the previous record.

Mb (Control) I don't know whether I can beat it or not; we'll see. (between 'ready' and 'go') I didn't think about the record. I was just trying to get the letters and numbers into a system that would make it easy to do.

La (Schizophrenic) When you asked me about the record it didn't make any impression. I didn't care about it, but I wanted to go as fast as I could. Just before you said 'go,' I remembered I had trouble with the 7s, so I added it to several other numbers for practice.

(3) 'S'=self-centered desire to excel. *S* is tense, almost worried. He is so much concerned with himself that he fails to plan the procedure. Frequently he has to reiterate the directions after the signal to start.

Ms (Control) . . . Gee, I wanted to beat that today, but when you said 'go' I found I didn't know what I was going to do, and I lost time right at the start. . . .

Si (Schizophrenic) I get worried before you say 'go' and then it is just like I used to be in school before an examination . . . (Anything else?) No. I forgot all about what I did last week when I got the picture of the numbers in my mind. . . .

Do (Paretic) When you say 'ready' I get nervous and forget what I am supposed to do.

(4) *'W'*=willing but not planned. *S* intends to do well, but he does not take the trouble to guard himself against the errors of previous performances.

Go (Schizophrenic) Just waiting, just waiting for the 'go.' (Did you do the best you could?) Sure, I always do my best.

Wb (Paretic) Waiting for you to say 'go' . . . what was there to think about?

(5) *'D'*=defeat accepted. *S* believes he cannot better his record. He may or may not plan. Occasionally, he changes this belief after he has entered upon the performance.

Wb (Control) For some reason I knew I wouldn't be any good at this today, maybe that's the reason I didn't do any better. All the time I was waiting for you to say 'go,' I was feeling how punk I can do sometimes.

Fr (Schizophrenic) I can't beat it; I'm no good at these things. I never was good . . . (between 'ready' and 'go') Nothing, I knew I couldn't beat it.

(6) 'O'=preoccupied. Thinking about something during the fore-period, *S* forgets all about planning. He may even forget the directions.

Mb (Control; the only control who reported such instances) I got to thinking about what Dr. X said and what Dr. Y said about my report . . . and I forgot all about this.

Ro (Schizophrenic) Guess I was thinking again. I don't know. I don't remember. Yes, I do. I was thinking about what H— said when I was up in the laboratory . . .

(7) 'I'=indifference. *S* cares neither about the record nor the task. He offers no opposition. He enters the task in a neutral state, merely following directions.

Ka (Schizophrenic) I don't care about my record. (between 'ready' and 'go') I wasn't thinking. (Did you do the best you could?) I did as much as I felt like, what difference is it whether I do better or worse.

(8) 'A'=antipathy for the task. *S* considers refusing to perform, but decides to comply.

Go (Schizophrenic) I don't like this counting, and I don't see why I should do it. It is better we talk.

Ka (Schizophrenic) I was thinking I would just not do it. . . .

The percentages of each *S*'s performances following each of these eight forms of preparation are shown in Table III.

The differences in preparation which are typical of the three groups stand out clearly in Table III. The controls more frequently entertain the competitive attitude and plan their performances ('C' and 'P'). A general shift from the competitive and planned types of preparation to 'S' and 'W' (self-centered and planless) characterizes the paretics. This difference between them and the controls is largely a matter of inability to remember directions. The paretics frequently lost the task even after they had given evidence of understanding the formal instructions; but this loss is not a result of self-instruction. They showed an intent to excel. Three of them (*Do, Gr,* and *Sm*), who were somewhat more deteriorated than the other two, usually wanted to start immediately after they understood the direction. When

TABLE III

FREQUENCY (PERCENTAGES) OF EACH FORM OF PREPARATION

	'C'	'P'	'S'	'W'	'D'	'O'	'I'	'A
Controls								
Eh	54	31	2	9	4			
Mb	7	54	0	27	5	7		
Ms	40	35	8	5	12			
Pe	72	17	8	0	3			
Wb	82	5	3	5	5			
Wo	50	27	8	0	15			
Ta	65	21	2	7	5			
Yo	52	15	3	13	17			
Schizophrenics								
Fr	3	34	0	6	31	13	10	3
Go	29	11	11	30	4	4		11
Ka				12			52	36
La	18	45	7	10	10		10	
Re	7	50	9	7	2	10	15	
Ro	28	15	15	20	4	18		
Si	7	16	23	8			37	9
Sz	58	10	5	22	5			
We	16	37	16	10		16	5	
Paretics								
Do	18	27	18	30	2			5
Gr	12	20	28	32	8			
Se	50	30	0	5	10	5		
Sm	25	7	48	16	4			
Wo	61	25	4	4	6			
Group Averages								
Controls	52.7	25.6	4.3	8.3	8.2	.9		
Schiz.	18.4	24.2	9.1	13.9	6.0	6.7	14.2	7.1
Paretics	33.2	21.8	19.6	17.4	6.0	1.0		1.0

required to wait through the fore-period interval, they forgot what they were about to do.

The tendency among the schizophrenics is to report a wide variety of preparations. Occasionally they are competitive; frequently they plan their performances with the intention of doing their best. But now and then they follow every other type of preparation. They almost exclusively report instances of preoccupation in the fore-period, indifference, and antipathy for the task. The tendency for their output to vary accords with this instable government.

The exceptions in Table III deserve separate consideration. The proportions of the various forms of preparation reported by the controls are fairly uniform for all save *Mb*, the *S* who appeared as the most variable of the controls in Table I. At the time the experiment was in progress this girl was under considerable strain. Her doctoral dissertation was in preparation, and she was actively in search of a position. These factors, combined with more personal complications, kept her in a predicament. She will again stand out as an exception when 'extraneous occupations' are considered.

The proportion of competitive and planned preparations reported by *Sz* is approximately that of the controls. He likewise exhibited low coefficients of variability in Table I. This does not, however, mean that he was a well person. He was dazed and confused much of the time. He had what he called 'blank periods' when he was unable to continue his performance for as long as 10 sec. This does not appear in his day-to-day variation, however, for these 'blank periods' came with fair regularity. He fought this confusion continually and sought methods of overcoming it, frequently questioning *E* about the difficulty. *Sz* presented a strikingly different picture in the experimental situation from that of *Ro,* another patient with the same diagnosis (hebephrenic schizophrenia).

Several other schizophrenic patients reported specific and characteristic types of preparation. About a third of the time *Fr* entered her performances with an acceptance of defeat. Her self-instruction for performance on these tasks is typical of her attitude toward life. *Ka,* always depressed, merely did what she had to do. Although her reports were not formal, and she grew to dislike the experiment, *E* never had great difficulty in getting her to describe her preparation. She said, "I just do it because I have to." (What did you think about between the 'ready' and the 'go'?) "I wasn't thinking, I was just sitting." Or, at other times, "I was just wondering why I do these things when I don't like them." The few times she performed willingly (*W*) she did not stop to plan her procedure. Her report then was, "I was just wondering how much I could do if I tried." In two of these instances she more than doubled her average performance. Witness her coefficients of variability for card-sorting (726) and substitution (240). This is a different sort of government from the 'blank periods' of *Sz,* which occurred under every kind of preparation, even the competitive and planned sorts. When *Sz* tried to describe the phenomenon, he could say only that he "went blank," that he "couldn't think."

The unusually large proportion (37%) of 'indifference' reported by *Si* is not quite typical of that category. Early in the experiment *Si* tended to be worried about how well she would perform (23% 'S'). This self-involved preparation precluded her planning a procedure. Consequently she often found herself confused, her poise gone in the midst of the performance. There was another anomaly in her self-government. If, during the performance, she happened to comment, "I'm going well, maybe I'll beat it," that was followed by the self-instruction, "When I think I'm doing well, I never am." As a result the performance was blocked. In a number of performances in which she had proceeded at the rate of more than one addition a second for 3 half-minutes, this *S* would suddenly appear to have lost poise and her output would drop to as low as 6 additions for the last half-minute. In order to avoid such situations, she finally instructed herself to be indifferent. As she described it, "I don't pay any attention to the record. I don't want to. If I beat my record it's all right. If I don't, I don't care." Curve I shows the trend of her output following this self-instruction.[8]

The tendency to be occupied with something besides the task during

[8] Curves of output plotted over profiles of the four factors of government in question here show clearly the relation between them and the quantity of output. Self-instruction in preparation appears as the most patent factor.

the fore-period appears in all but three schizophrenic patients. One normal and one paretic also reported fore-periods of preoccupation. *Mb's* thoughts during this period concerned her work; *Se's* concerned what he had to do on the way home from his visit to the hospital. The extraneous occupations of the schizophrenics, as will be shown, are apparently unrelated to the task or to matters of immediate concern.

(c) Course of performance. *S* was requested to report the course of his performance, after the several preparations previously described, in order that any change of self-instruction or any anomalous factor might not escape description. This proved the hardest part of the report to classify. What has come out are seven characteriza-

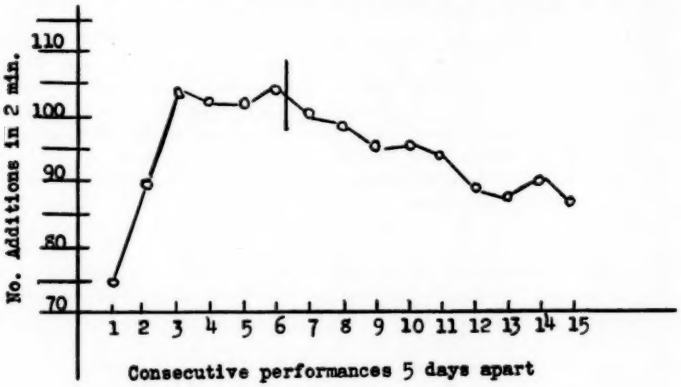

Fig. 1. Output of *Si* before and after self-instruction to 'be indifferent'

tions. Most of these follow directly from *S*'s own preparation. Since the reports are long, a summary of each characterization is given.

(1) 'E': *S* pushes himself to the limit. A skilled performance where the steps occur almost automatically. It follows the competitive sort of preparation.

(2) 'S': Like 'E,' but lacking zest. It follows the planned but non-competitive type of preparation.

(3) 'C': Lacks skill, but *S* is working to the best of his ability. A concentrated performance in which the steps are more or less explicitly directed. It may or may not have the zest of the competitive attitude, and may follow any of the first four types of preparation.

(4) 'B': The instructions are confused. *S* finds it necessary to re-orient himself, or he continues to the end not knowing whether he is proceeding correctly. Only performances on card-sorting and substitution are found in this category, which follows any of the unplanned types of preparation.

(5) 'D': Disturbed poise. It is akin to what is known to the hunter as 'buck fever.' *S*'s self-object somehow becomes involved in the situation so that he cannot proceed. It was reported by *S*s of all three groups following all of the first five types of preparation. Never did it follow a fore-period when *S* was indifferent, preoccupied, or opposed to performing. Perceiving an error as *S* approached his record, while working under pressure, was given most frequently as the basis

for this phenomenon. The case of patient *Si* has already been cited. Among the schizophrenics the bases were several. Something extraneous might be the basis, or, as in the non-typical case of *Sz*, loss of poise might follow a 'blank period.'

(6) 'I': An indifferent performance, one in which *S* simply followed the demands of the situation, himself contributing as little as possible. It follows foreperiods where the preparation has been 'indifference,' 'preoccupation,' or 'opposition to performing.'

(7) 'G': Works grudgingly, barely coöperating. As will appear in Table IV (where percentages for each type of performance are given) such performances occurred with only two *Ss*, *Ka*, a schizophrenic, and *Do*, the paretic girl.

TABLE IV

FREQUENCY (PERCENTAGE) OF EACH TYPE OF PERFORMANCE

	'E'	'S'	'C'	'B'	'D'	'I'	'G'
Controls							
Eh	33	24	33	5	5		
Mb	13	38	37	5	2	5	
Ms	30	25	30	12	3		
Pe	36	14	34	16			
Wb	35	17	33	5	10		
Wo	20	30	37	5	8		
Ta	65	15	17	0	3		
Yo	33	5	50	5	7		
Schizophrenics							
Fr	3	24	40	10		23	
Go	16	18	42	11	5	8	
Ka	0	0	8	4		60	28
La	17	28	40	5	7	3	
Re	4	18	44	2	14	18	
Ro	32	18	30	15	3	2	
Si	4	23	25		28	20	
Sz	36	10	38	8	8		
We		16	58	5		21	
Paretics							
Do	0	0	69	25	4		2
Gr	13	20	62	3	2		
Se	25	33	37	5			
Sm	21	5	65	9			
Wo	41	25	20	11	3		
Averages							
Controls	33.1	21.1	33.8	6.6	4.8	0.6	
Schizo.	12.4	17.2	36.1	6.7	7.2	17.2	3.1
Paretics	20.0	16.6	50.6	10.6	1.8		0.4

Excepting the disturbances of poise ('D'), one might have predicted the percentages in Table IV from those in Table III. Having planned their procedures less frequently than did the controls, the schizophrenics and paretics show a smaller percentage of the skilled performances ('E' and 'S') and more of the concentrated type ('C'). That there is not a larger percentage of confused performances ('B') for the schizophrenics than for the controls is possibly accounted for by the prevalence of 'indifference' in the schizophrenic's preparation. The prevalence of unspirited ('I') performances was to be expected.

(d) Extraneous occupations. One theory of schizophrenia holds these persons to be living in a world of their own imagination. Accordingly, the patient comes out of his reverie only to meet the most urgent demands of life. In order to check this possibility, we asked all *Ss* to report any occupation extraneous to the task. From the reports nine categories of occupation emerged.

(1) 'N': Nothing. *S* has been entirely engrossed with the execution of the task. There were such reports as, "I didn't have time to think of anything else."

(2) 'A': Occupations associated with the task. This category includes all normal comments on the material and past incidents recalled by the situation. Here are a few reports.

Mb I thought of *S*— and all the trouble she had classifying her stuff, and I began to wonder what you were going to do with this.
Go Of a time in school in Germany when we had arithmetic.
Si I suddenly began to think about my room in the fifth grade when we had long division. The teacher. . . .

(3) 'S': New tasks are set. *S* decides to determine whether there is an order in the answers to the addition problems, or to determine whether there is the same number of substitutions to be made within each line. These are reported by both schizophrenics and controls, but very seldom by paretics.

Eb I set myself to see whether all the lines had four substitutions. When I had found only three in one line and five in another, I decided I was wrong.
Pe I began to wonder how these instructions were changed from day to day, (card-sorting task) and I began to recall what they had been on previous days, then I decided I had better forget about that and speed up.
Re It suddenly occurred to me that you must have a system. I remembered that somebody told me once that there was a system in all tests.
We I began to wonder where I had done this sort of thing before, then I remembered it was in a test I took one time.

(4) 'D': Distractions perceived. *S* is disturbed by a noise in the hall or by some movement of the experimenter.

(5) 'I': Immediate plans. The plans of the day crowd in for a moment, or something which has recently occurred is considered. Here are reports.

Mb Suddenly it occurred to me not to forget to take that paper down to my room.
Ro I was just thinking that I had to go down in the basement for H— before I went to work.
Se I was just thinking about what station I should get off at to get that. . . .

(6) 'R': Incidents and topics with no obvious or reportable connection with the task or the situation. Whether the connection is merely lost, or whether the topic is part of the day's reverie cannot be told. From our protocols they appear simply as occupations with no relation to the situation.

Fr That time my brother brought that hitch-hiker home and she ran away with that lady's jewels and fur coat.
Go Why should people eat meat? Isn't it wrong to eat. . . .
Re I was just seeing little sparks fly; I used to like to do that when I was a little girl.

(7) 'U': Unpleasant social incidents. These are also unrelated to the situation, but they are more understandable. *S* relives a situation damaging to his self-object.

Fr It was the time Mr. F— told me he would lock me up if he caught me going to that woman (a fortune teller).
Go It is a big party. . . . I can tell she doesn't like me.
Ro I was wondering whether I could ever be a man. . . . That time I caught that guy stealing. I was scared to tell and I was scared not to tell. I don't know how to take care of myself.

(8) 'F': Fleeting, undescribable occupations. Whether *S* is unwilling to admit

some of these or they have really escaped him, we could not tell. The category includes such reports as:

Pe There was something else. I wasn't really concentrating on the task, but I can't tell what it was.

Re I guess I was thinking. But I don't really think, I just have half thoughts and they go away before I can tell what they are.

TABLE V

FREQUENCY (PERCENTAGE) OF EACH TYPE OF EXTRANEOUS OCCUPATION

	'N'	'A'	'S'	'D'	'I'	'R'	'U'	'F'	'H'
Controls									
Eh	68	3	23	4	2				
Mb	45	10	17		25		3		
Ms	45	10	10	25	10				
Pe	66	14	17					3	
Wb	40	10	36	7	7				
Wo	62	25	5	5				3	
Ta	70	21	2	5	2				
Yo	73	10					17		
Schizophrenics									
Fr	37	3	5	3	8	23	18	3	
Go	32	9	5		4	30	20		
Ka	56	12	4		4	12		12	
La	42	38	3	8	5	2	2		
Re	26	17	17		5	12	2	21	
Ro	22	20	3		8	22	15	10	
Si	48	5	17	5	13	2	8	2	
Sz	84	7	2	2	3	2			
We	33	5	22		6	2	20	6	6
Paretics									
Do	82			2	16				
Gr	95			2	3				
Se	95				5				
Sm	100								
Wh	96				4				
Averages									
Controls	58.6	12.9	13.7	5.8	5.8		2.5	.7	
Schiz.	42.2	12.9	8.7	2.0	6.2	11.9	9.4	6.0	.7
Paretics	93.6			.8	5.6				

(9) 'H': Hallucinations. Only one of these patients (*We*) reported hallucinations while working. In these instances, she heard *E* saying things which were not said.

The tendencies in Table V are clear. The normal-controls are occupied with the situation at hand. So are the paretics, but the schizophrenics are characteristically preoccupied with matters entirely unrelated to the situation and frequently of a personal nature.

Again the two schizophrenics with the lowest coefficients of variability (*La* and *Sz*) appear most like the normals. *Ka* also reports nothing extraneous for a large percentage of the performances, but here one cannot rely too much upon her reports. *E* never had her complete confidence.

It is interesting to note that the schizophrenics are no more distractable by things in the surroundings than are the normals or paretics. The extraneous occupations emerge from the organism.

A large percentage of unpleasant social incidents reported by *Fr, Go, Ro,* and *We*

is evidence of suffering from social maladjustment. It is peculiar, however, that *La,* a paranoid whose history contained evidence of continuous social maladjustment, reported so few. One might account for it by assuming that her self-control had not been greatly deranged; she was not as grossly disordered as the others.

The reports of the paretics are also illuminating. They appear almost always to be entirely engrossed with the task at hand. It would seem that the cortical loss suffered may have decreased the range of their psychological activities. Reports by *Sm,* however, cannot be considered reliable here. His memorial functions had been considerably reduced. Although he always recognized *E,* toward the end of the experiment he disclaimed having seen him more than once before.

(3) Quantitative influence of reported factors. The controls and the paretics have both been shown to be less variable in output than the schizophrenics; so, likewise, have they differed from the schizophrenics in their fore-period preparation and in the amount and type of extraneous occupations reported. Still to be shown is the effect of mood, of preparation, and of the extraneous occupations upon the output. Since the character of the performances as gleaned from the reports of their course turned out to be largely dependent upon *S*'s preparation, treatment of the characterizations of the course of the performance is omitted. For facility in handling, the various forms of preparation and of extraneous occupation were regrouped into three categories, as mood has been. The three categories of preparation are: (1) competitive ('C') and planned ('P'), (2) the self-conscious desire to excel ('S') and the attitude of willingness to perform ('W'), and (3) including acceptance of defeat ('D'), the state of preoccupation ('O'), indifference ('I'), and antipathy for the task ('A'). Extraneous occupations were regrouped into: (1) nothing ('N'); (2) things associated with the task ('A'), self-imposing of additional tasks ('S'), perceived distractions ('D'), and immediate plans ('I'): and (3) all the 'remote occupations' ('R,' 'U,' 'I' and 'H').

The measures of output for each schizophrenic patient on every task have been separated according to the category (1, 2 or 3) of mood, of preparation, and of extraneous occupation to which they belong. Let us consider only mood for purposes of describing the statistical procedure. The scores, for each patient, falling within each of the three given categories were averaged. Then *the differences* were computed between the patient's average performance (1) in high spirits and neutral spirits (I-II), (2) in high spirits and low spirits (I-III), and (3) in neutral spirits and low spirits (II-III). A group-difference was then found by obtaining the mean of the individual differences. The significance of these mean differences was tested by means of the formula: $t=\bar{x}/(s/n)$ where $S^2/n=[n\Sigma(x^2)-(\Sigma x)^2]/n^2(n-1)]$.[9] The same procedure was followed with preparation and with extraneous occupations. The results are shown in Table VI.

Each of the three factors under consideration tends to have its effect upon the patient's output, but the effect of his preparation is greatest, as shown by the mean differences between types of preparation, and is most consistent as shown by the 'P.' In only two instances (Adding; I-III and II-III) are the differences occasioned by mood statistically reliable.[10] There are likewise only two instances in which dif-

[9] R. A. Fisher, *Statistical Methods for Research Workers,* 1930, Cf. chapter on tests for the significance of means; for 'P,' see p. 139.

[10] Fisher considers a 'P' of 0.05 significant, meaning that there is only 1 chance in 20 that that difference would occur gratuitously.

TABLE VI

EFFECT OF MOOD, OF PREPARATION, AND OF EXTRANEOUS OCCUPATIONS UPON OUTPUT

Task	Type	Mood			Preparation			Extraneous occupations		
		Mean Diff.	t	P	Mean Diff.	t	P	Mean Diff.	t	P
Adding	(I–II)	10.8	3.109	.1	7.8	3.342	.02	9.2	2.745	.05
	(I–III)	16.0	3.822	.01	18.6	3.668	.01	12.3	2.838	.05
	(II–III)	8.6	2.996	.05	13.0	2.731	.05	8.3	2.067	.1
Card-dealing	(II–I)	.3	.028	1.	.7	.610	.6	.6	1.051	.4
	(III–I)	1.8	.654	.5	1.9	1.249	.3	2.0	1.333	.3
	(III–II)	2.9	1.590	.2	1.7	1.359	.3	2.6	1.741	.2
Card-sorting	(II–I)	2.8	1.390	.3	6.6	3.015	.02	5.9	2.157	.1
	(III–I)	12.6	2.397	.1	23.3	4.151	.01	16.1	1.874	.2
	(III–II)	11.7	2.302	.1	16.0	2.744	.05	5.1	1.594	.2
Substitution	(I–II)	25.0	1.187	.3	87.0	2.826	.05	−1.5	.649	.6
	(I–III)	58.0	1.471	.2	90.0	5.062	.01	66.0	2.110	.1
	(II–III)	33.0	.727	.5	10.0	.638	.6	75.0	2.056	.1

Terms of scores:

Adding = number of additions in 2 min., errors subtracted.

Dealing and sorting cards = time required for the deck, 1 sec. added for each error.

Substitution = number of letters examined in 2 min., with ten subtracted for each error.

ferences related to extraneous occupations are statistically significant. On the other hand, the differences in output resulting from planned or unplanned forms of preparation (excepting those related to dealing cards which is a simple motor task) are all significant.

If a choice can be made, extraneous occupations would seem to have the second most potent influence upon *S*'s output. The interrelationships here, however, cannot be clarified from these data. It is possible that the patient's mood may result from his extraneous occupations. Conversely, the reverse may be true.

TABLE VII

INFLUENCE OF MOOD UPON THE TYPES OF PREPARATION, OF PROCEDURE, AND OF EXTRANEOUS OCCUPATIONS

(Reported in percentage)

		Preparation			*Procedure*			*Extraneous Occupations*		
Subj.	Mood	I	II	III	I	II	III	I	II	III
Fr	H									
	N	41	18	41	18	41	41	41	18	41
	L	0	0	100	0	10	90	22	0	78
Go	H	56	32	12	44	31	25	31	6	62
	N	34	41	25	32	34	34	39	13	48
	L	41	33	26	17	48	35	8	0	92
La	H									
	N	69	23	8	58	42	0	42	54	4
	L	45	23	32	36	36	28	38	50	12
Re	H	100	0	0	66	34	0	33	43	24
	N	62	24	14	50	33	17	35	38	27
	L	33	27	40	27	40	33	22	39	39
Ro	H	63	37	0	63	12	25	50	25	25
	N	43	43	14	55	37	8	27	42	31
	L	57	14	29	42	33	25	25	12	63
Si	H	55	11	34	29	21	50	43	43	14
	N	40	20	40	36	20	44	58	26	16
	L	40	20	40	38	15	47	37	21	42
We	H	50	50	0	25	75	0	50	50	0
	N	27	55	19	48	26	26	37	18	45
	L	0	50	50	0	50	50	0	0	100

(4) Effect of mood upon preparation, procedure, and extraneous occupations. We asked whether the patient's mood would affect the other elements in his report. In order to answer the question, the percentages of each of the three categories of preparation, of procedure, and of extraneous occupations, which occur with each of the three types of mood were computed. These results appear in Table VII.

Variations of mood appear to have some effect. For every patient who reported differing moods there is a tendency for a larger proportion of planning for the performance when his spirits are high. Likewise, there is a greater tendency for 'indifference' and other adverse forms of preparation (type III) when his spirits are low. A similar change in proportions occurs for the characteristics of performance. As has been noted before, these follow from the shifts in preparation.

High spirits apparently have very slight influence upon extraneous occupations. There is no tendency for *S* to report fewer extraneous occupations when in high spirits, but he reports a smaller proportion of 'remote' occupations (III). Low moods, on the other hand, are definitely accompanied by an increased proportion of these 'remote' occupations.

These data do not, however, tell which is cart and which is horse. They say nothing of whether mood or occupation is primary, but they do state a tendency. High moods and neutral moods tend to be accompanied by more planning of procedure than do low moods, and *S* tends to report a larger proportion of remote occupations when his spirits are low.

Discussion

What do these results mean? From the standpoint of procedure, they demonstrate a way in which the method of report may be utilized in experimental studies with disordered persons. At least some schizophrenic patients, although their reports are not formal, can and do give illuminating accounts of the instructions under which they are operating. The paretic's report is likewise useful, but the paretics who performed in this investigation were either less skillful in reporting or they had less to report than did the schizophrenic patients.

The significance of a larger than normal proportion of instances of preoccupation, indifference, and accepted defeat in the schizophrenic's preparation is difficult to state. The preponderance of these types of preparation might be considered as evidence that the schizophrenic lives in a dream world, is preoccupied with his own personal arrangement of life, or that he is emotionally apathetic. Because of the differences between patients, however, it is evident that these pictures of schizophrenia are inadequate. The fact that there are such marked differences between patients of the same sub-type (witness *Ro* and *Sz*, who were both diagnosed as schizophrenic) indicates that these categories have little psychological significance.

The difference in the manner in which schizophrenics and paretics entered upon these tasks does provide us with a psychological distinction between these two disorders. The paretic, unless he is too greatly deteriorated, is still interested in improving upon his own performance. He may find it difficult to understand some of the instructions. Every paretic in this group had to be coached to put the cards below six on the one side and the cards above six on the other. Once he got the directions, the paretic wanted to begin his performance. He was impatient about waiting through the interval between 'ready' and 'go.' Forced to wait, he was frequently unable to start. The directions had to be repeated. In spite of these losses in the realms of comprehension and memory, which appear suggestively similar to those which occur following the ablation of the frontal cortex

in apes,[11] the paretic performs under a self-instruction to excel. This instruction is not always explicit, but these paretics almost never admitted defeat nor were they ever indifferent. They were set to beat their records. The schizophrenics, on the other hand, vary considerably from time to time in this set to excel. This very factor of instable government distinguishes them from the paretics and from the normals.

The lack of interest in his own performance, which we have been describing as characteristic of all but two of our schizophrenic patients, has usually been attributed to a lack of motivation. Such a statement is merely a classificatory subterfuge. One understands the phenomenon no more clearly after making the statement than before. The results of this investigation appear to indicate that this 'lack of motivation' is self-instructed. Self-instructed does not mean that the schizophrenic is malingering. Neither does it mean that he knows the relationships among his various attitudes. Rather is he in a prison the walls of which are his beliefs about himself. They govern his functions in such a fashion as to make him uninterested in his performances and in life.

When extraneous occupations are considered, we find another psychological difference between schizophrenics and normals, and between schizophrenics and paretics. The paretic reports almost nothing extraneous. One might consider this an indication that the virtuosity of his psychological activity has been reduced by losses of cerebral tissue. The normal person reports extraneous occupations which are closely related to the task or his immediate plans. He is held in check by his relations with objects and persons in his immediate temporal environoment. The schizophrenic patient again shows evidence of a derangement in his government. He concerns himself, even during a task, with incidents, usually in his biography, which have no relation to the situation in which he finds himself. The interrelationships are not clear regarding the factors governing him, but they offer large descriptive possibilities.[12]

Summary

This investigation was undertaken to determine whether and how schizophrenic patients differ from normals and from paretics in some of the more accessible governors of psychological activity. Mood, preparation for

[11] C. F. Jacobsen, The effects of extirpation of the frontal association areas in monkeys upon complex adaptive behavior, *Amer. J. Physiol.*, 109, 1934, 59.

[12] It is still possible that the phenomena which we have described are ultimately to be glandular imbalance or some other specific organic factor. Our patients are too narrowly selected to draw conclusions regarding the whole body of persons diagnosed as schizophrenic. This investigation has demonstrated, however, that an approach can be made to the description of governing factors which appear in reportable form.

performance, procedure, and extraneous occupations have been considered.

The *Ss*, 8 non-psychotics, 8 schizophrenics, and 5 paretics, carried out four tasks (adding, card-dealing, card-sorting, and substitution) at intervals of 5 days. Fifteen samples of work were obtained from most of the *Ss*. The method consisted in having *S* compete with his own previous performances, and, by means of the method of report, determine time by time his mood, his preparation, his procedure, and his extraneous occupations.

From the results the following conclusions may be drawn.

(1) The method of report is feasible for the purpose of determining the sets and self-instructions of many schizophrenic and paretic patients. The paretics employed in this investigation were probably less reliable than the schizophrenics.

(2) Schizophrenics, paretics, and normals cannot be differentiated by their reports of mood.

(3) The schizophrenics' output under the three gradations of mood is not significantly different, but mood does tend to affect the reports of preparation, of procedure, and of extraneous occupations.

(4) Although the schizophrenic may plan his performances and even adopt a competitive attitude, his reports indicate that he frequently enters upon the task accepting defeat, preoccupied with other things, or entirely indifferent to the outcome of his performance. This tendency differentiates him from the normal *Ss* and from the paretics, who are set to give their best performance.

(5) The schizophrenic's output is significantly decreased by lack of plan, and again by such adverse preparation as that just mentioned. The variation in his preparation largely accounts for the variation in his output.

(6) The schizophrenics reported more extraneous occupations than did the controls. The latter, in turn, reported more than did the paretics. While the paretic appeared to be entirely engrossed with the task at hand, the schizophrenic tended to be frequently preoccupied with things having no relation to the situation.

(7) An analogy is drawn between the tendency of paretics to forget the task and to begin the performance immediately upon understanding the instructions and the changes which occur in the adaptive behavior of apes when the frontal 'association areas' are removed.

(8) This investigation demonstrates that, whether the theory of schizophrenia indicated at the beginning of this paper is true or not, it is possible to obtain a description of the variable instability of the schizophrenic patient's self-government from an experimental situation and procedure.

AN EYE-MOVEMENT STUDY OF STEREOSCOPIC VISION

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The problems of stereoscopic vision and depth perception were investigated as early as the year 300 B.C. by Euclid.[1] One problem that especially puzzled early writers was the illusion of the moon at the horizon. This subject was discussed as early as the year 150 A.D. by Ptolemy.[2] No attempt will be made, however, to cover the early literature on depth perception as that would carry us far afield. The present review is limited to the more recent work which deals especially with the problem of eye-movements during stereoscopic vision.

(1) Factors influencing stereoscopic vision and depth perception. The problem of 'corresponding retinal points' and the location of the horopter has caused a great amount of controversy: recent experimental data throw considerable doubt on the theory of 'corresponding points,' especially in any strict sense of the term. Ames, Ogle, and Gliddon,[3] as a result of a series of carefully designed experiments, believe that mathematical formulae may be calculated for the empirical horopter. Their plots of the projections of 'corresponding retinal points' changed with certain known and unknown conditions. Their particular interest was in the influence of the relative size and shape of the retinal images; they found that differences in this respect caused definite changes in the position of the horopter. They conclude that "it would appear that the fundamental hypothesis of specific directional values associated with specific brain centers is placed on a firmer basis." This conclusion seems hardly compatible with the findings of Lashley and others in regard to brain functioning.

Glanville believes that no one has yet made a satisfactory determination of the horopter.[4] His results, obtained from 5 *O*s who made over 900 binocular observations, failed to demonstrate any fixed region corresponding to the Müllerian circle wherein the objects are seen single and outside of which they appear double. For this reason he calls into question the significance of the doctrine of corresponding points. Glanville believes that a "group of central factors" is more important than retinal correspondence or non-correspondence.

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[1] As cited by C. A. Wood, editor, *The American Encyclopedia and Dictionary of Ophthalmology*, 16, 1920, 12.175.

[2] As cited by Helmholtz, *Physiological Optics*, English transl., 3, 1925, 360.

[3] A. Ames, K. N. Ogle, and G. H. Gliddon, Corresponding retinal points, the horopter, and the size and shape of ocular images, *J. Opt. Soc. Amer.*, 221, 1932, 536-631.

[4] A. D. Glanville, The psychological significance of the horopter, this JOURNAL, 45, 1933, 592-628.

McAllister[5] and Dodge,[6] working independently and using different techniques, found that the image of a point fixed does not fall on any particular point on the retina, but that it may fall on a considerable area around and about the fovea centralis. Their results show that there is no 'law of fixation,' and that fixation is more or less random. Dodge concludes that a functional center in the sense of a retinal point does not exist. These results cast considerable doubt on any theory of strict retinal correspondence.

Another question which has caused controversy is the relative importance of accommodation and convergence in the perception of depth. This problem is difficult to attack experimentally because of the reciprocal influence of these two functions as described by Sheard[7] and Tait.[8] This has been studied by Swensen,[9] who dissociated these two functions by the use of a modified Wheatstone stereoscope. The convergence could be adjusted at any point desired by the proper positioning of the two barrels of the instrument. Accommodation could be adjusted to the proper distance by sliding the tubes within the barrels. The *O* was required to set a pointer at the same distance as a line which appeared in the instrument. It was found that when accommodation and convergence were dissociated, the stereoscopic object was always placed between the accommodation and convergence distances, and in every case set closer to the convergence distance. Measuring the errors showed that accommodation possessed only about one-third of the power of convergence at the distances investigated.

(2) Eye-movements during stereoscopic vision and depth perception. The question of the importance of eye-movements and convergence during stereoscopic vision has been discussed by a number of writers, and numerous opinions have been expressed. Brücke[10] believed that the normal procedure of seeing an object solid consists of a rapid series of eye-movements during the course of which the various parts of the solid object are successively fixated. Dove[11] performed experiments in 1841 which he believed completely disproved the eye-movement theory. He made exposures of stereograms, using a spark-gap which presented the stimulus for only a minute fraction of a second. He reported stereoscopic effects during this brief exposure, which excluded the possibility of eye-movements. On the basis of Dove's findings numerous retinal pattern theories of stereoscopic vision were proposed.

Dove's experiments, however, cannot be said to be decisive. Conclusive evidence refuting these results was found in 1910 by Von Karpinska.[12] She made a study of

[5] C. N. McAllister, The fixation of points in the visual field, *Psychol. Monog.*, 7, 1905, (no. 29), 17-54.

[6] Raymond Dodge, An experimental study of visual fixation, *ibid.*, 8, 1907, (no. 35), 1-95.

[7] C. Sheard, On accommodative exophoria, or the accommodation associated with the act of convergence, *Amer. J. Physiol. Opt.*, 1, 1920, 234-249.

[8] E. E. Tait, A reciprocal reflex system in the accommodation-convergence relationships, this JOURNAL, 45, 1933, 647-662.

[9] H. A. Swensen, The relative influence of accommodation and convergence in the judgment of distance, *J. Gen. Psychol.*, 7, 1932, 360-380.

[10] As cited by Helmholtz, *op. cit.*, 454.

[11] As cited by J. W. Baird, The influence of accommodation and convergence on the perception of depth, this JOURNAL, 14, 1903, 150-200.

[12] L. von Karpinska, Experimentelle Beiträge zur Analyse der Tiefenwahrnehmungen, *Zsch. f. Psychol.*, 57, 1910, 1-88.

the impressions of stereograms using a spark gap similar to Dove's, but her *O*s did not know on any trial whether the picture would be flat or stereoscopic for both kinds were used. Under these conditions one spark was not enough to produce stereoscopic effects.

Washburn has recently made two studies bearing on this point. Her experiments show the importance of retinal rivalry in stereoscopic vision and the relation between the fusion of stereoscopic pictures in the original impression and in the negative after-image. Using 19 *O*s, the time was recorded when the fusion of two pictures of a cone occurred.[13] If the inner circle of the cone appeared in the center of the outer one, binocular vision was present. If the inner circle appeared to the left, the right eye was being used, and if the inner circle appeared to the right, the left eye was being used. The time that the circle appeared in the center in the original impression was compared with the time that this occurred in the negative after-image. For 15 of 19 *O*s, the binocular fusion occurred for a longer time in the original impression than in the negative after-image; in one case 100 times as long. These data show that normal stereoscopic vision is a much more complex process than the simple interpretation of two retinal patterns.

Washburn believes that the perception of solidity is a distinctly motor experience and also suggests the possibility that binocular rivalry is important in normal binocular vision. In support of this she describes an experiment in which 21 *O*s observed a cube in a dark room.[14] The *O*s fixed a point on the edge of the cube, one side of which was not illuminated and painted black; the other side was painted white and illuminated. With all of the *O*s it was found that the size of the gray patch seen subjectively between the two faces appeared to change, showing a distinct binocular rivalry. On the basis of this experiment alone it cannot be said that a similar rivalry occurs continuously during normal stereoscopic vision, but it suggests that such a condition is a possibility, especially when other facts of binocular rivalry are considered. This has also been suggested by Gassendus.[15]

Peckham has made a study of eye movements in alternation during binocular vision.[16] His method involved the direct observation of monocular horizontal movements. The observations were made through a telescope while the *S* reported the fluctuations in perception between the two eyes. The figures were observed in a precision Wheatstone stereoscope, and the *S*s' reports of 'areal disappearance' were recorded on a polygraph. Using this method, Peckham found no correlation between eye-movements and alternation.

Judd performed an experiment which is unique in that it is the only one which has made a strictly objective approach to a problem very closely related to stereoscopic vision and depth perception.[17] Judd made a photographic study of convergence and divergence movements, using an improved kinetoscopic camera. The

[13] M. F. Washburn and D. L. Smith, Stereoscopic binocular fusion in the original impression and in the negative after-image, this JOURNAL, 45, 1933, 320-321.

[14] M. F. Washburn, Retinal rivalry as a neglected factor in stereoscopic vision, *Proc. Nat. Acad. Sci.*, 19, 1933, 773-777.

[15] As cited by J. W. French, Stereoscopy restated, *Amer. J. Physiol. Opt.*, 7, 1926, 250-300.

[16] R. H. Peckham, Eye-movements during 'retinal rivalry,' this JOURNAL, 48, 1936, 41-63.

[17] C. H. Judd, Photographic records of convergence and divergence, *Psychol. Monog.*, 6, 1907 (no. 34), 370-435.

convergence and divergence movements were made between two fixed points in space. The distance of the two points was so arranged that the movements of convergence would be clearly marked. General randomness and lack of harmony of the eye-movements proved to be the outstanding characteristics exhibited by the *Ss*. Some *Ss* moved the right eye faster than the left, and some moved the left faster than the right. Judd showed also that the eyes sometimes make conjugate movements just prior to convergence movements, and that during convergence there is a tendency to overshoot the mark. He concluded that binocular vision is the result of the coördination and unification of various percepts.

The Problem

A review of the literature on binocular phenomena in general and stereoscopic vision and depth perception in particular, shows that most of the investigations employed purely subjective techniques. The studies of Judd and Peckham may be considered as exceptions. Peckham's problem, however, did not involve depth perception, although it did throw light on certain problems of binocular vision. Judd's work involved simple convergence and divergence movements while the eyes alternately fixed two points in space. Swensen settled the old problem of the relative importance of accommodation and convergence in the perception of depth by showing that convergence is approximately three times as important as accommodation, but he did not show how it is important. Washburn proposed a motor theory, but she had little objective evidence to support it. Peckham found no relation between eye-movements and alternation in binocular vision, but his measurements were made monocularly and in one meridian only.

Although the problem of stereoscopic vision is fundamentally sensory, in this study objective data, which bear directly on the question, have been obtained. Such findings have been secured by recording the eye-movements of individuals while they were observing stereograms. The specific purposes of the present study may be stated as follows.

(1) To devise a mechanism that will simultaneously record the vertical and horizontal movements of both eyes and magnify the movement sufficiently to show any small shifts made by the eyes during stereoscopic vision.

(2) To ascertain whether there is any difference between the fixation of isolated points and the same points in context.

(3) To determine whether stereoscopic convergence and divergence movements are similar to the convergence and divergence movements reported by others.

(4) To analyze stereoscopic fixation and compare it with non-stereoscopic fixation.

(5) To determine the place of eye-movements in stereoscopic vision and depth perception.

Apparatus. The apparatus used to photograph the eye-movements has been briefly described elsewhere by Clark.[15] The instrument permitted the vertical and horizontal movements of both eyes to be photographed on a single record with high magnification of movement. The displacement of the corneal image was magnified 14 times. Head-movements were also recorded. Time was measured in 1/25 sec. by the use of an episcotister driven by a synchronous motor.

A Wheatstone stereoscope was constructed and mounted on the camera in such a way that the eye-movements could be photographed over the top while the eyes looked slightly down into it. The stereoscope was made up of two mirrors which were mounted on a pivot to permit free rotation. The frame was made of light

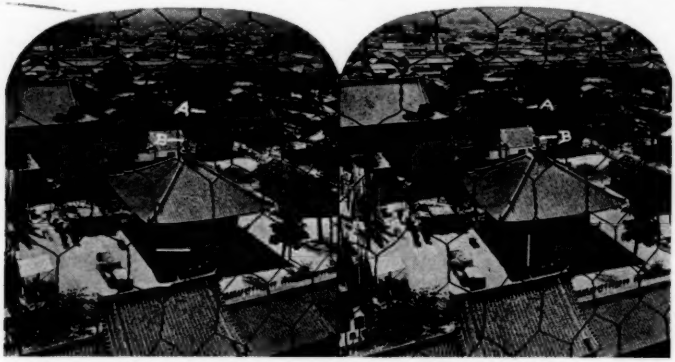

FIG. 1. STEREOGRAM USED IN THE EXPERIMENTS
(Keystone View Company, 086-C, 3919, Looking from Tennoji Pagoda west over Osaka, Japan. The *Ss* fixed alternately points A and B.)

veneer, and the mirrors were placed at an angle of 45° with the body of the instrument. The stereograms fitted into slots in the two ends of the instrument, and were about 19 cm. from the axis about which the two halves rotated. The stereograms were approximately 310 cm. from the *Ss*' eyes, and both pictures were adjustable vertically to correct any false vertical placement. Each stereogram was illuminated by a 15 c.p. 6 to 8 volt lamp operated at 9 volts. This gave sufficient illumination to overcome the light used to illuminate the eyes in order to photograph them.

Experimental procedure. The stereogram (see Fig. 1) used throughout the experiments was chosen chiefly for two reasons. First, the picture presented an object (the screen) which appeared to be quite close to the *S;* and secondly, it also contained another object, the ball on top of the building in the near distance, which could be easily located and was not likely to be confused with any other point in the picture while the test was being made. The points, A and B (Fig. 1), were used as the fixation-points. They required, in order to be 'fixed,' movements of

[15] B. Clark, A camera for simultaneous record of horizontal and vertical movements of both eyes, this JOURNAL, 46, 1934, 325-326.

convergence and divergence of a relatively high degree with a small amount of vertical movement. In every case *S* began at A and shifted his fixation back and forth between these two points.

Points A and B were also reproduced on a heavy white cardboard to determine any difference between eye-movements with and without context. A majority of the *Ss* experienced considerable difficulty in 'fusing' the points out of context, in fact some of them were unable to do so at all. For this reason records were not obtained for all *Ss* while they 'fixed' these points.

The eye-movements of 16 *Ss* were photographed while they were observing the stereoscopic material. The *Ss* were 10 undergraduates and 6 graduates in psychology. All were men. Records were obtained from everyone using the stereoscopic picture, and for 13 during the fixation of the isolated points. Over 3,000 ocular movements were photographed.

The testing period lasted approximately 30 min. for each *S*. He was first instructed as to the general nature of the experiment. After he had set the stereoscope at the most satisfactory position, he became acquainted with the picture and the points of fixation. He was then given several trials in shifting from one point to the other while *E* observed his eyes directly. *E* counted as follows: "One, two, three, ready shift," keeping time with a metronome beating 100 times per min. At 'three' the shutter on the lamp was closed and opened rapidly, giving a zero mark on the film. At the command 'shift' *S* changed his fixation from one point to the other and held it there until the next command to change.

When *S* understood what was required of him, he took his position and the instrument was focussed. He was then allowed a rest of a minute or two and permitted to ask questions. The spectacle frame used to indicate head-movements was then put on, and the images brought into focus. The lamp was then stepped up, the film started and an exposure made. Two separate records were made for the complex picture and two for the isolated points. After each exposure *S* was permitted to rest. Some of the records taken during the fixation of the points were not used because the *Ss* were unable to 'fuse' them, although they had no difficulty in 'fusing' the identical points in context. Some parts of the records were ruined by *S*'s blinking, but this rarely occurred.

After each trial *S* reported whether he was successful in performing the procedure correctly, and whether he accurately 'fixed' the points indicated: if he had not fulfilled both of these requirements, the record was discarded.

Treatment of the data. In order to facilitate the examination of the data, the photographic records obtained from the *Ss* were plotted on quadruled paper. In many cases the two lines on the film were not exactly the same size, due to the lack of a perfect focus. Actual measurement of the displacement for each movement eliminated the possibility of misjudgment in comparing the records.

No data were recorded that showed head-movements corresponding to the eye-movements, although it would probably have been valid to use such records because, even though the head moved, stereoscopic vision was reported subjectively, and the eyes were moving in a way common in ordinary stereoscopic vision. The head-line was used as a base-line to determine the displacements on the records,

as it indicated the course over which the paper was moving. Lines were drawn parallel to this base, and the displacements were measured in relation to these parallels. No displacements of less than ½ mm. were recorded.

The data for this study were all obtained from the graphs, which were all rechecked at least once. On several occasions when the graph was doubted, a second recheck showed it to be correct. Any deviation of less than ½ mm. or approximately 20′ of ocular rotation was considered as a part of the fixation. The deviation varied slightly from *S* to *S* due to differences in focus and also as a result of the variation in the center of rotation of the eye, as has been shown by Verrijp.[19]

Results. (1) A comparison of ocular movements 'fixing' isolated points and points in context. A qualitative examination of the data showed that, in general, the stereoscopic fixation movements were very much like the fixation movements described by McAllister[20] and Dodge,[21] and the convergence movements were similar to those described by Judd.[22] This was true both with and without context. Random movements during fixation were evident even from a casual examination of the graphs. These movements ranged from 0.3° to 1.2° in magnitude. Uniform or symmetrical movements, either in the vertical or horizontal meridians, were the exceptions rather than the rule. This was shown by all of the *S*s, and the asymmetrical movements occurred both in the horizontal and vertical meridians. Two examples from individual records will exemplify this. The record of one *S* showed that the right eye remained fixed for 10/25 sec., and in the same period of time the left eye made 4 horizontal movements within an area of 0.6°. For another *S* within a period of 8/25 sec. the right eye showed no vertical movement although there was a 0.3° rotation to the right. During the same period the left eye showed no horizontal displacement at all, but made a 0.6° rotation upward.

Wide individual differences were found in the way the eyes 'fixed' the points, as well as the way in which they moved from one point to another. The same thing held true for identical convergence and divergence movements of an individual at different times, *i.e.* the eye-movements of the same *S* varied considerably on successive trials in which the same points were successively 'fixed.' The magnitude of the movement, as well as the general method of movement, varied on successive repetitions of the same convergence and divergence movements.

Judd reported that his *S*s showed a tendency to move the eyes in the same direction laterally, prior to convergence adjustments. In other words, associated movements of the two eyes preceded convergence or divergence movements. This was also found to be the case during stereoscopic vision, but it was far from being consistent. With the fixation of points in context, 50% of the movements showed associated movements prior to the divergence movements, when the eyes shifted from the wire to the ball. Such reactions were made only 19% of the time in chang-

[19] C. D. Verrijp, Ocular movements, *Arch. Ophth.*, 4, 1930, 73-83.
[20] *Op. cit.* [21] *Op. cit.* [22] *Op. cit.*

ing the fixation from the ball to the wire. While changing fixation between the isolated points, 31% of the trials exhibited associated movements prior to divergence movements. On the other hand, in moving from the distant to the near point, only 21% of the adjustments showed associated movements prior to convergence. It would appear from the preceding results that these incongruous associated movements occur more frequently during divergence movements.

An analysis of the vertical component of the movement showed that it was sometimes preceded by a fixation of relatively long duration; at other times it was preceded by a slow movement in the same direction as the major movement; and sometimes it was preceded by a movement in the opposite direction to the correct movement. These movements were

TABLE I

A Comparison of the Average Number of Eye-Movements Made per Second While the Eyes "Fixed" Isolated Points and Points in Context

	Horizontal component		Vertical component	
	Right eye	Left eye	Right eye	Left eye
Isolated point A	7.55	7.01	4.11	6.20
Point A in context	6.88	7.59	3.88	6.39
Difference	0.67	0.58	0.23	0.19
D/σ diff.	0.70	0.38	0.59	0.20
Isolated point B	6.98	6.66	4.28	5.53
Point B in context	7.08	6.55	3.83	5.44
Difference	0.10	0.11	0.45	0.09
D/σ diff.	0.01	0.61	0.13	0.02

sometimes symmetrical in the two eyes, but in most cases they were very dissimilar.

In connection with the associated movements of the two eyes, it is important to know which eye makes the correct shift during the associated movement, when both eyes should move in opposite directions. This determination was made from the graphs and compared with the results obtained from the Miles V-scope test for eye-dominance.[23] Considering the data as a whole, in 75% of the movements the non-dominant eye followed the dominant eye first and then made the corrective movement. It will be remembered, however, that this included only those cases in which the correct movement was not made immediately.

The graphs were also examined to determine whether or not there was a consistent leading eye in the movements from one point to another. The vertical and horizontal components were considered separately. Fixing the points in context, one eye or the other led horizontally 88% of the time

[23] W. R. Miles, Ocular dominance as demonstrated by unconscious sighting, *J. Exper. Psychol.*, 12, 1929, 113-126.

within the limits of the test (1/25 sec.). In the vertical meridian one eye led only 14% of the time. Using isolated points, one eye or the other led horizontally 78% of the time and vertically 22% of the time. The leading eye did not correlate significantly with the dominant eye.

A comparison was made of the number of movements for the fixation of the two different types of material. In order to do this, the vertical and horizontal components were considered separately, and the movements were counted, beginning 15/25 sec. after the peak of the major vertical movement was reached. Table I summarizes these results. They show a slightly greater number of movements during the fixation of isolated points, but the difference is not statistically reliable.

It can be concluded from these results that both qualitatively and quantitatively there is little if any difference between the stereoscopic fixation of isolated points and points in context, in spite of the fact that the *S*s reported a considerable amount of difficulty in 'fusing' the isolated points. Random movements were common throughout the fixations, and symmetrical movements were by far the exception rather than the rule. The eye-movements of the same *S* varied considerably in successive trials. Incongruous movements were common, *i.e.* associated or divergence movements occurred prior to a correct convergence movement, and associated or convergence movements often occurred just prior to a correct divergence movement.

Within the limits of the test there was no consistent leading eye, although one eye or the other led a considerable part of the time. Comparing the fixation of points with and without context, there was no statistically reliable difference between the number of movements made. The most uniform thing about the eye-movements seemed to be their consistent asymmetry and irregularity.

It is important to note that the depth effects in the picture were exceptionally pronounced. The wire appeared to stand out just in front of the eyes, and the buildings appeared solid. This was reported by all of the *S*s, even by one who did not pass the Verhoeff tests.[24] On the other hand, the isolated points gave practically no depth effects, but the eye-movements were the same as far as could be determined from the graphs.

It is difficult to see how kinaesthetic sensations from the extra-ocular muscles could be of any major importance in stereoscopic perception, in view of the fact that the eye-movements were so consistently irregular

[24] F. H. Verhoeff, A simple test of stereoscopic vision, *Amer. J. Physiol. Opt.*, 7, 1926, 383-385; An improved and new test for stereoscopic vision, *Amer. J. Ophth.*, 16, 1933, 589-591.

during the fixation of points in and out of context. Therefore, these findings throw into question theories that attempt to explain stereoscopic vision and depth perception on the basis of kinaesthetic cues. This raises the question of the importance of eye-movements, a point that will be discussed in detail later.

(2) Stereoscopic fixation and the 'law of corresponding points.' The work of McAllister[25] and Dodge [26] casts doubt on the 'law of corresponding points,' especially in any strict sense. Dodge believes that the important question in psychological optics is not the fixation point but a relation between areas of greater or less distinct vision. A modification of the theory of 'corresponding points' is to be found in the so called Panum's areas. This concept, that there are not corresponding points but corresponding retinal areas and any image falling within these areas will be seen single, is more in accord with known disparities of fixation.

The results obtained in these experiments threw light upon this particular problem. The data were studied from two points of view. In the first place, the graphs were examined to determine how much binocular variation was shown in the fixation of the points. Secondly, an examination of the vertical components of the movement showed a considerable amount of 'drifting' back toward the point previously 'fixed' while *S* reported that he was 'fixing' one point accurately. The amount of this 'drift' should give a measure of the range of the central retina which may 'fix' a point, *i.e.* the size of the retinal area having a single directional value.

The variation of the fixation between the two eyes was determined by measuring the difference between the widest and the narrowest parts of the curves, beginning 15/25 sec. after the peak of the major movement was reached. The measurement was made for both the vertical and horizontal components and for the fixation of points with and without context. This measure gives, in degrees, the widest variation between the two eyes around a point. It shows the area on the central retina on which points may be imaged and still be seen as single, *i.e.* the dimensions of Panum's area.

The average for 16 Ss was found to be 44.4 $\pm$ 13.2′ for the binocular vertical displacement and 48.4 $\pm$ 13.4′ for the binocular horizontal displacement. The monocular displacement would, of course, be half of this. The largest displacements, however, are of the greatest significance, because they show the limits on the retina within which points may be imaged and still be reported as single subjectively. The largest monocular displacement, assuming that the displacement was equal in the two eyes, was 1.2°. In other words, the binocular disparity was as much as 2.4° and still single vision was reported subjectively. Horizontally, the greatest monocular disparity was found to be 0.9°.

An analysis of the vertical 'drifting' movement, after the movement between the two points, also showed a wide area around about and including the fovea centralis which may be used to 'fix' a point. After 85% of the movements there was a corrective or 'drifting' movement back toward the first fixation point within 5/25 sec.

[25] *Op. cit.* [26] *Op. cit.*

after the peak of the vertical movement was reached. In some cases this was a corrective movement due to the fact that the first movement was too large, but at other times not. The graphs were examined to determine how much angular variation occurred. The average was found to be 45.6 ± 15.1′. In individual cases the variation was as much as 2.0°.

These data show that a considerable area of the central retina may be used to 'fix' a point, and that impressions falling within 1°–1.5° from the center of the fovea may be seen single during normal stereoscopic vision. It will also be noted that this region includes a considerable area outside of the fovea centralis, which is from 30′ to 50′ in size (Helmholtz). These disparities in correspondence should not be compared with the stimulation of similar parts of the retina over an extended period of time, as when a prism is placed before one eye. In this case the disparity lasts many times longer, and if the prism is strong enough, diplopia will occur. This, in effect, moves the object in relation to the eye, and a corrective movement may be impossible. On the other hand, the fixation disparities during stereoscopic vision are very brief, and the image moves to another part of the retina in a very short period of time.

These data show that the so-called 'corresponding points' cover a considerable area of the retina, and that it cannot be said that stereoscopic vision and depth perception are the result of the stimulation of specific points on one retina or corresponding points in the two eyes. Although these results apply directly to the central retina, there is every reason to believe that the area would be as large in the peripheral parts, if not larger, due to the great reduction in acuity. There is no doubt that an horopter, an area in which points are seen single, can be located in the visual field and that its position will vary with certain factors which have been pointed out by Ames, Ogle, and Gliddon.[27] To state that specific retinal points and specific brain centers are associated with specific directional values is, however, incompatible with the findings of this study, although such a belief has been stated by numerous authors, *e.g.* Ames, Ogle and Gliddon,[28] Helmholtz,[29] Sheard,[30] and Tscherning,[31] in spite of the early work indicating marked fixation disparities.

(3) The relation between eye-movements and stereoscopic vision. One possible clew to the difference in depth values of two points is to be found in different kinaesthetic cues during far and near fixations. In order to test this possibility, the curves were examined to determine whether any qualitative or quantitative differences occurred between far and near fixations.

Qualitatively, there appeared to be little difference between the fixation of far and near points in stereograms. Random movements were present in both cases.

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[27] *Op. cit.* [28] *Ibid.* [29] *Op. cit.*
[30] C. Sheard, *Physiological Optics*, 1918.
[31] H. Tscherning, *Physiologic Optics*, trans. by C. Weiland, 1924, 324.

Uniform and symmetrical movements were the exception rather than the rule, and the method of fixation varied between individuals and for the same individual during successive fixations of the same point. In other words, qualitatively, the two types of fixation and convergence movements were the same as far as could be determined from the graphs. A comparison of the speed of convergence and divergence movements seemed to indicate that, in general, convergence movements were more rapid than divergence movements, although exceptions were numerous. No strictly quantitative measure of this was possible because, due to the 'random' movements, it was very difficult to determine just when the convergence movement ended. The setting of the stereoscope would also have some effect on this.

The number of movements made during far and near fixation of the points in the stereoscopic picture was also calculated. The count was made beginning 15/25 sec. after the peak of the major movements between the two points was reached. Only

TABLE II

AVERAGE NUMBER OF MOVEMENTS PER SECOND MADE BY THE EYES FIXING POINTS IN A STEREOGRAM

S	Fixating near point		Fixating far point		S	Fixating near point		Fixating far point	
	R	L	R	L		R	L	R	L
1	5.9	9.9	10.2	13.8	9	5.3	8.1	3.3	7.0
2	5.1	5.8	6.0	6.2	10	4.1	7.0	11.9	9.2
3	11.8	13.7	15.9	14.1	11	9.0	6.8	8.2	7.2
4	7.7	9.2	9.1	9.9	12	5.8	2.4	3.1	3.8
5	7.1	9.8	8.9	12.0	13	8.3	6.9	8.8	10.2
6	9.8	7.2	8.7	10.9	14	11.1	9.9	10.0	9.1
7	9.0	11.7	5.9	5.8	15	5.2	7.1	5.1	5.2
8	7.9	8.3	6.2	10.8	16	—	—	8.8	11.8

relatively rapid movements, 0.3° in 2/25 sec. or less, were counted, although slower movements were rare and would have affected the average but slightly. The great majority of these movements occurred in 1/50 sec. or less. The horizontal and vertical components were considered together. A complex movemnt, *e.g.*, a movement up and to the right, or down and to the left, was considered one movement, as were simple horizontal and vertical movements. The data for the individual *Ss* are summarized in Table II. The average number of movements made during the fixation of the distant and near objects was found to be 8.89 ± 2.11 movements per sec. The average number of movements during the fixation of the ball was 9.22 per sec., and during the fixation of the screen the average number of movements per sec. was 8.45. The difference divided by its sigma was 0.17, indicating 560 chances in 1000 of a true difference—practically chance.

To summarize these results: there is no reliable difference between eye-movements made during the fixation of far and near points in stereograms. This evidence and the data discussed in the preceding sections show that kinaesthetic sensations from the extra-ocular muscles cannot be important in stereoscopic vision and depth perception. If eye-movements are important in stereoscopic vision and depth perception, their importance is

not in the sensations of ocular movements which result from the constant ocular tremors.

This raises the question of the importance of eye-movements in stereoscopic perception. In relation to this problem there is another important question. Is there perception during these movements? Dodge[32] points out that perception is definitely lowered during saccadic movements in which the angular velocity is relatively large (5° in 28 σ). In pursuit movements there is, however, relatively clear vision during the movement. While the eyes are converging, Dodge says there is relatively clear vision, for these movements are relatively slow. Dodge[33] found that whereas the velocity of saccadic movements is of the order of 10° in 40-50 σ, movements of convergence and divergence have a velocity of the order of 10° in 400 σ. He reports them as long as 10° in 1 sec. These results are closely in accord with the data found in the present investigation. As Dodge[34] points out, however, these movements permit only more or less clear perception of the field of vision.

Eye-movement records taken by Metfessel and the writer during stroboscopic vision caused by impressed ocular movements have important bearing on this problem.[35] The angular velocity of these movements was approximately 10° in 700 σ. It was shown by Metfessel that a disk having 50 or 100 radii drawn on it will appear stationary when revolving 60 r.p.m., if the forced vibration causes a complete cycle of the eyes 50 times per sec. Warren has made an analysis of this stroboscopic effect and has shown that there is only one possible time when the stroboscopic perception can take place, and that is when the eye is moving with the revolving lines.[36] In other words, the stroboscopic perception comes when the eye is moving with the disk and the relative angular velocity between the eye and disk is practically zero. On the other hand, when the eye is moving in the direction opposite to that of the disk, the perception is greatly reduced, resulting in a blur. It is evident that the perception is not reduced to zero because, if a small point of light is observed while the eyes are vibrated, a blurred line or oval is seen rather than a series of dots. The angular velocity of the eye-movements during stereoscopic fixation is of the order of 10° in 700 σ, which is approximately equal to the relative velocity between the eye and disk during stroboscopic vision. This indicates that although the movements during stereoscopic vision are slow enough to permit vision, they do not allow clear perception.

On the basis of these findings, it can be concluded that stereoscopic vision involves the perception of a series of intermittent, continuously changing retinal impressions received from the stereograms. These oscillations between clear and blurred vision are the result of the tremulous movements of the eyes occurring on the average

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[32] Raymond Dodge, Visual perception during eye movement, *Psychol. Rev.*, 7, 1900, 454-465; The illusion of clear vision during eye movement, *Psychol. Bull.*, 2, 1905, 193-199.

[33] R. Dodge and T. S. Cline, The angle velocity of eye movements, *Psychol. Rev.*, 8, 1901, 145-157.

[34] R. Dodge, Five types of eye movement in the horizontal plane of regard, *Amer. J. Physiol.*, 8, 1903, 307-329.

[35] M. Metfessel, Stroboscopy by means of impressed eye movements or mirror vibration, *Science*, 78, 1933, 416-417.

[36] N. D. Warren, An explanation of Metfassel's stroboscopic effect in mirror vibration, *J. Gen. Psychol.*, 10, 1934, 463-465.

about 8 times per sec. The clear perception occurs during the fixation, and between fixations there are periods of lowered acuity of vision. An examination of the graphs also shows that the clear perception may be binocular, but for a great part of the time it is monocular; *i.e.* the clear perception shifts from one eye to the other. These movements present a series of rapidly changing impressions of the stereograms, which give cues for the perception of solidity and depth.

It is a well known fact that similar slower changes are of great assistance in depth perception. A common elementary laboratory experiment gives a good example of this. If monocular depth perception is tested first with the head fixed and then with the head moving from side to side, it is clear that monocular parallax is a great aid in determining depth differences. Chavasse has shown the importance of this oscillation in fish.[27] In spite of the fact that they have monocular vision, fish have a keen sense of depth as shown by their accuracy in striking. A rapid monocular parallax is caused by the swimming movements. A similar thing is true of birds.

(4) A theory of stereoscopic vision. These results lead to a theory of stereoscopic vision which is based fundamentally on a rapid visual oscillation not only of the eyes together but from one eye to the other. This causes a series of intermittent retinal impressions which are of basic importance in stereoscopic vision and depth perception. The most important part of this fluctuation is the change in clear vision from one eye to the other. The resulting effect is similar to monocular parallax, but the change is from one eye to the other and has the distinct advantage of being very rapid. It is significant that the perception depends upon stimulus changes that are below the threshold, *i.e.* the *S*s are entirely unaware of any change or blurring. In other words, stereoscopic perception depends upon the organization and unification of minimal changes. If the proper organization, 'closure' occurs, stereoscopic perception results, but if the organization is absent, stereoscopic effects are either small or absent.

Such a theory conforms to the facts of monocular and binocular fixation disparity better than a 'law of corresponding points,' even when the 'points' are considered as areas. Panum's areas, instead of being regions that, when simultaneously stimulated, permit clear vision, become areas within which intermittent stimuli are seen single. 'Corresponding points' lose their significance.

The relative independence of the two eyes has been suggested by Graham,[28] for he could not demonstrate binocular summation in his studies of retinal rivalry. Dawson[29] has also stressed visual oscillation on the basis of his studies of binocular and uni-

[27] B. Chavasse, The nature and antiquity of stereopsis, *Trans. Ophth. Soc. United King.*, 13, 1931, 268-286.

[28] C. H. Graham, An investigation of binocular summation: I. The fovea, *J. Gen. Psychol.*, 3, 1930, 494-510; II. The periphery, *ibid.*, 5, 1931, 311-328.

[29] S. Dawson, Binocular and uniocular discriminations of brightness, *Brit. J. Psychol.*, 6, 1913-14, 78-108.

ocular discrimination of brightness. Washburn[40] has also suggested that alternation between the two eyes may be important in stereoscopic vision. The oscillations which these authors describe are, however, many times slower than the ones to be found in these experiments, and they are based on different phenomena.

Washburn's study of stereoscopic vision in the original impression and in the negative after-image gives further evidence which supports, indirectly if not directly, the importance of successive retinal impressions.[41] For 15 of her 19 *Os*, binocular fusion occurred for a longer time in the original impression than in the negative after-image; in one case 100 times as long. These data show that stereoscopic vision cannot be explained on the basis of the interpretation of two isolated retinal patterns. Emphasis should be placed on the fact that a series of relatively complex visual patterns is very important in stereoscopic perception.

These findings lead to the conclusion that the great advantage of binocular vision in stereopsis is due to the rapid oscillation of the fixation of the two eyes, and the inordinate alternation of clear vision from one eye to the other. This causes a series of intermittent retinal impressions which are of fundamental importance in normal stereoscopic vision. Kinaesthetic sensations, both from the ciliary and extraocular muscles, give cues which are of less importance. Convergence movements themselves could only give cues which are of relatively small importance, for these movements occur rarely as compared to the 'random' movements which occur over an extended period of time. Therefore, stereoscopic vision is to be explained fundamentally as due to an organization and unification of these inordinate retinal impressions and not simply in terms of the stimulation of specific geometrical points on the retina.

Summary and Conclusions

An examination of the literature on stereoscopic vision and depth perception revealed almost a total absence of objective studies of these phenomena. It was the purpose of this investigation to obtain strictly objective data bearing on the problem. In order to do this, an eye-movement camera was used, and the vertical and horizontal eye-movements were recorded while the *Ss* observed stereoscopic material in a Wheatstone stereoscope.

Sixteen university men acted as *Ss* for the experiment. The eye-movements of these men were photographed while they 'fixed' alternately two specific points in a complex stereoscopic picture. Over 3000 ocular movements were photographed. The points were so selected as to require a maximum of convergence and divergence during the change. In order to determine the influence of context on the eye-movements, the identical points were reproduced on white cardboard. Thirteen of the same *Ss* were also photographed while they alternately 'fixed' these two points.

An examination of the results of this investigation leads to the conclusion that stereoscopic perception is to be explained fundamentally as the result of the cerebral organization and unification of a series of inter-

[40] *Opp. citt.*
[41] *Op. cit.*, this Journal, 45, 1933, 320 f.

mittent retinal impressions which occur as clear vision changes from one eye to the other. These oscillations, which cause a diminution in vision, are caused by eye-movements during fixation. Kinaesthetic impressions from the extraocular or ciliary muscles, and the stimulation of 'corresponding retinal points' are of much less importance. These conclusions are drawn on the basis of the following specific findings.

(1) The velocity of the 'random' movements during stereoscopic fixation is sufficient to cause a definite diminution in vision.

(2) Neither qualitative nor quantitative differences are found in comparing the fixation of near and far points in stereograms.

(3) Qualitatively and quantitatively there is little, if any, difference between the fixation of isolated points and the same points in context.

(4) The 'law of corresponding points' is not in harmony with the findings of this study, for impressions which fall within a region of 1°–1.5° from the center of the fovea are seen single during normal stereoscopic vision.

(5) Any position within an area of 1°–1.5° from the center of the fovea may be used to 'fix' a point.

(6) Randomness and irregularity of the movements are outstanding characteristics exhibited by the *S*s.

(7) Uniform or symmetrical movements are the exception rather than the rule.

(8) Wide individual differences in the behavior of the two eyes are found.

(9) Wide variations are found in successive performances of the same individual.

(10) Within the limits of the test, there is no correlation between the dominant and the leading eye in ocular movements.

THE COURSE OF RELAXATION IN MUSCLES OF ATHLETES

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Individuals vary greatly in ability to relax quickly and completely and to maintain this state in a particular muscle-group.[1] Only a small percentage of 21 presumably healthy university students succeeded in relaxing the muscles that flex the forearm as promptly and completely as is typical in subjects previously trained to relax.[2]

How do athletes relax, as compared with other untrained students and as compared with trained subjects? This question is investigated in 10 athletes, 5 of them members of the University of Chicago football team.

The earliest investigators on speed of relaxation in man (Orchansky,[3] Gad),[4] using chiefly mechanical devices, measured the interval from the signal to relax to the moment when contraction begins to subside. They called this the "reaction-time for relaxation" and concluded that it is approximately equal to the "reaction-time for contraction." In later years, Voerckel[5] using the string galvanometer, measured from the signal to relax to the moment of complete cessation of action-potentials, as shown by her instrument. She called this interval the "reaction-time for relaxation" and concluded that it is approximately equal to the "reaction-time for contraction," when the subject has been permitted sufficient practise. In the present investigation, the interval between the signal to relax and the moment of complete relaxation in the muscle tested will be called *complete relaxation-time* (c.rel.t.). This must be distinguished from the interval between the same signal and the moment when relaxation begins (b.rel.t.). Voerckel fails to make this distinction clearly. Without doubt, b.rel.t. rather than c.rel.t. should be compared with what has been classically known as reaction-time for contraction, since the latter has always meant the interval which ends at the moment when contraction *begins*.

Mechanical devices have furnished only rough determinations for b.rel.t. Accurate determinations by the action-potential method require that the initial moment of slump be clearly detectable in the photographic records. Hitherto this has not been accomplished. Furthermore, early investigators could not determine accurately

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[1] E. Jacobson, Electrical measurements concerning muscular contraction (tonus) and the cultivation of relaxation in man; relaxation-times of individuals, *Amer. J. Physiol.*, 108, 1934, 573-580.

[2] E. Jacobson, *Progressive Relaxation*, 1929, 40-100.

[3] J. Orchansky, Zur Lehre von der Willenstätigkeit, *Arch. f. (Anat. u.) Physiol.*, 1889, 173.

[4] J. Gad, Über die Reaktionszeit für Erregung und für Hemmung, *Du Bois Reymond's Arch.*, 1887, 363.

[5] H. Voerckel. Reaktionszeit der willk. Kontraktion und Erschlaffung der Beuger und Strecker des Vörderarmes, *Zsch. f. Biol.*, 75, 1922, 79.

the moment when relaxation becomes complete; for slight contraction lingers on in many *Ss*, as shown by the persistence of action-potentials of low voltage, not detectable by the instruments of a former day. Accordingly it seems of interest to secure data on relaxation-times with improved instruments and also, as will presently appear, to indicate the course of relaxation by plotting action-potentials against time.

Method. The conditions of experiment have been previously described.[6] A box with high walls as well as a floor of soft iron lined with copper screening encloses the couch and the *S* lying upon it, except that the cover is always kept open in the present studies. *S* is instructed to bend his right arm at once when a telegraph key clicks; to relax as quickly and completely as possible after a second click. The interval between signals does not exceed 2 or 3 sec. Preliminary practise is given until it is certain that *S* understands what he is to do. Only slight bending is desired—about half-an-inch of movement of the hand. If the sounder is located at some distance from *S*, he sometimes reacts to sounds other than the signal—whether these sounds occur by accident or by design. This is effectively avoided if the sound is quite distinctive and is within a few feet of the ear. The present purposes do not require us to determine by further procedures whether *S* would distinguish between true and false or irritative signals, as is customarily done in certain investigations on reaction-time.[7] Commonly 4 or 5 tests are made with an *S*, and the results are taken as one rough but fairly significant indication of this individual's success at relaxation. The first test is discarded if it differs very greatly from those which follow, as occurred in one *S*. Repeating the tests many more times would be requisite for a statistical treatment of results. But with such repetition, reduction of relaxation-time would tend to occur more or less progressively, as *S* became skilled at this particular reaction. The results would then be a measure or a function of the skill attained upon such continued practice rather than an indication of *S*'s ability to relax at a particular time prior to practice.

We have said that the mechanical devices developed up to date for the measurement of contraction in intact man fail to prove sufficiently reliable and accurate for present purposes. Accordingly we employed an amplifier-galvanometer assembly.[8] Its extreme sensitivity—along with relative quietness of base-line—has made possible for the first time accurate measurements of slight muscular contractions. Measurements can be made within fractions of one microvolt. So far as we know, such slight transient voltages have not before been recorded in any laboratory, physiological or physical.

String tension in the galvanometer is so adjusted as to yield an excursion of 3 or 4 cm. per millivolt applied to the string terminals. This provides a fairly flat characteristic of the response to frequencies up to about three hundred or more.

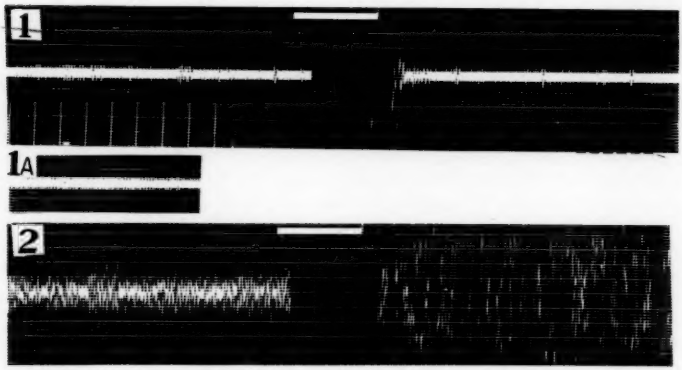
A platinum wire electrode ('positive') inserted about 1 cm. through the skin into the right biceps-brachial muscle tissue is connected to the inner winding of the input

[6] Jacobson, *op. cit., Amer. J. Physiol.*
[7] W. Wirth, Die Reaktionzeiten, *Handbuch der Physiologie*, 1927, 525-599.
[8] Changes made in the amplifier circuit to eliminate underdamping will be discussed in a subsequent article.

transformer. The other electrode is inserted under the skin in the elbow pit and is connected to the outer winding.

In some of the present tests, the photographic paper moves about 2 in. per sec.; in others about 10 or more times as fast. As a control test, it is always essential to secure a record of the base-line, produced upon short-circuiting the input terminals of the amplifier. Other precautions have been sufficiently detailed in recent articles on electrical measurements concerning muscular contraction.[9]

The magnitude of action-potentials is calculated as before from the length of the



FIGS. 1–2. ILLUSTRATIVE RECORDS OF RELAXATION IN ATHLETES

Figs. 1 and 2 are read from left to right. In the left lower corner of Fig. 1, the interval between vertical time lines = 0.2 sec. Figure 1A is a control test showing the magnitudes of vibration of the string shadow (approximately vertical lines) upon short circuit across the input terminals of the amplifier. Accordingly these magnitudes are subtracted from the magnitudes observed in Figs. 1 and 2 in order to determine the results of tests on subjects. Distance between horizontal lines, 1 mm. = 3.5 microvolts (millionths of a volt). The initial downstroke of the signal line at the top of each figure marks the click-signal to bend the right arm promptly, while the final upstroke marks the click-signal to relax promptly.

Fig. 1 illustrates fair (although not perfect) relaxation in the arm flexor muscles of *S Cu* prior to the signal. He bends his arm promptly and relaxes fairly promptly, maintaining a relaxed state in the subsequent period.

Fig. 2 illustrates a contracted (tense) state of the arm flexor muscles in *Sm* prior to the signal. He bends his arm promptly and to a certain extent relaxes promptly; but that extent is highly limited, for action-potentials persist as seen in the approximately vertical lines exceeding the limits of the record. This record illustrates marked failure to attain relaxation.

approximately vertical lines produced on the photograph by the vibrations of the string-shadow. Obviously any particular line is the resultant of the voltages present at the moment in tissues or fluid in contact with the electrodes. Simultaneously positive and negative voltages in these tissues or fluid tend to offset each other. This

[9] E. Jacobson, Electrical measurements concerning muscular contraction (tonus) and the cultivation of relaxation in man: studies on arm flexors, *Amer. J. Physiol.*, 107, 1934, 230-248.

means a certain incalculable inaccuracy in the method of electrical measurement of muscle contraction, but for present purposes the error appears negligible.

In determining from the photographs the magnitude of potential difference in the electrodes at any instant, we again find the longest line occurring at that instant or within a small fraction of a second from that instant. Greatest accuracy would require us to take precisely the same fraction for all readings; but for present purposes it seems sufficiently accurate to make the reading at or within about 1 mm. (0.02 sec.) of the point representing any instant.

The approximately verticle lines are here measured from peak to peak and the overall voltage (V_{2m}) thus recorded, rather than from zero potential to one peak (V_m), as was done in two preceding articles. Accordingly the values in those articles must be multiplied by two to make them equivalent to present terms.

TABLE I

SHOWING FOR 4 Ss THE REACTION-TIMES AND THE BEGINNING RELAXATION-TIMES

Subjects							
Ad		*Ny*		*Al*		*Ku*	
Reaction	b.rel.t.	Reaction	b.rel.t.	Reaction	b.rel.t.	Reaction	b.rel.t.
.10	.14	.30	.12	.46	.11	.36	.11
.24	.10	.13	.10	.26	.13	.16	.11
.09	.16	.15	.25	.21	.15	.33	.17
.09	.09	.11	.23	.18	.14	.19	.16
—	—	.15	.16	—	—	—	—
Av. .13	.12	—	—	Av. .28	.13	Av. .26	.14
		Av. .17	.17				
				Av. of Av.....		.21	.14

Results. Illustrative photographic records of relaxation in athletes are presented in Figs. 1 and 2. In Fig. 1, the muscle is fairly well relaxed when the signal to contract occurs. Contraction follows in less than 0.2 sec. Upon the signal to relax, the action-potentials subside to zero value in less than 0.4 sec. The error does not exceed 0.2 or 0.3 microvolt, which is doubtless quite negligible. A different picture is presented in Fig. 2. This *S* fails to relax adequately even *before* the occurence of the signal to contract. The action-potentials remain up to 5.0 microvolts, in contrast with the approximately zero values shown before the first signal in Fig. 1. Upon the first signal, contraction follows in considerably less than 0.2 sec. Within less than 0.2 sec. partial relaxation follows the second signal, but action-potentials generally exceed 14.0 microvolts during the following 30 sec. of observation. These results illustrate something of the differences found in the ability of two football players to relax. The first achieves relaxation with almost the skill of a man previously trained to relax; while the other makes a prompt beginning at relaxation but otherwise fails markedly.

Table I presents data to illustrate reaction-times and beginning relaxation-times from 4 athletic *Ss*. Here the photographic paper was exposed at the rate of about

TABLE II

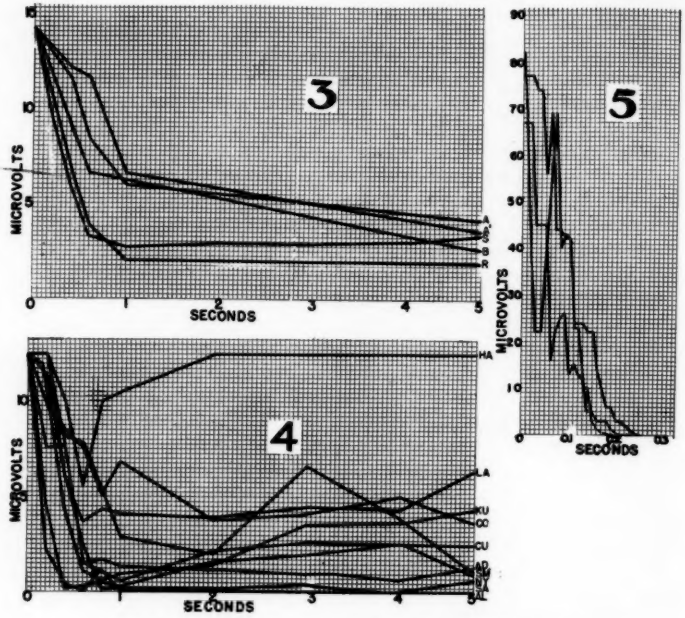
SHOWING FOR ALL THE Ss ILLUSTRATIVE DATA ON TIMES OF REACTION AND RELAXATION

S		Reaction-time (sec.)	2nd Signal to minimum V_{2m} (sec.)	Minimum V_{2m} (microvolts)	Duration of minimum V_{2m} (sec.)
		0.20	1.00	6.30	19.00
Co		0.30	1.20	6.30	28.00
		0.44	2.00	2.00	23.80+
	Av.	0.31	1.40	4.87	23.60+
		0.44	29.20	8.90	76.60+
Sm		0.36	0.80	1.40	31.60
		0.24	0.60	2.90	73.80+
	Av.	0.35	10.20	4.40	60.67+
		0.24	0.80	0.52	8.00+
		0.36	0.60	0.80	10.00
Ad		0.20	0.60	1.00	1.50
		0.20	0.48	1.00	1.50+
		0.20	0.45	1.00	1.50+
	Av.	0.24	0.59	0.86	4.62+
		0.40	0.52	0.50	4.40
		0.36	0.40	0.60	2.00
Ku		0.36	0.24	1.10	1.80
		0.34	0.30	3.20	2.20
		0.31	0.67	1.80	2.00+
	Av.	0.35	0.43	1.44	2.45+
		0.20	0.52	.00	0.23
		0.20	0.36	.00	0.53+
Sa		0.20	0.68	0.30	8.00
		0.30	0.72	.00	4.00
		0.35	0.30	.00	0.10
		0.25	0.46	.00	1.89+
	Av.	0.25	0.51	0.05	2.43+
		0.18	0.30	0.50	0.60
		0.12	0.24	0.50	2.20+
Ny		0.18	0.18	0.50	7.40+
		0.18	0.20	0.50	6.40+
		0.68	0.51	.00	1.45+
		0.38	0.40	.00	1.17+
	Av.	0.29	0.31	0.33	3.20+

TABLE II (*Concluded*)

S	Reaction-time (sec.)	2nd Signal to minimum V_{2m} (sec.)	Minimum V_{2m} (microvolts)	Duration of minimum V_{2m} (sec.)
	0.60	14.60	8.40	5.50
Ha	0.40	20.00	2.50	1.00
	0.22	0.50	2.50	0.30
Av.	0.41	11.70	4.47	2.27
	0.16	0.22	.00	5.60
	0.10	0.32	1.80	3.00
	0.20	0.30	.00	7.40+
Cu	0.14	0.28	.00	1.00
	0.16	0.24	.00	7.20+
	0.14	0.30	.00	6.00+
	0.14	0.46	0.50	1.13+
	0.34	0.29	0.50	1.03+
Av.	0.17	0.30	0.35	4.05+
	0.30	0.40	.00	5.00+
	0.24	0.30	.00	7.40+
	0.18	0.36	.00	8.00+
	0.10	0.40	.00	5.40
	0.20	0.50	.00	2.40+
Al	0.14	0.40	.00	5.50+
	0.20	0.36	.00	4.40+
	0.20	0.32	.00	2.80+
	0.14	0.06	.00	1.30+
	0.65	0.56	.00	0.65
	0.35	0.40	.00	1.80+
Av.	0.25	0.37	0.00	4.06+
	0.20	1.80	2.00	0.60
	0.28	13.60	3.00	6.20+
	0.16	0.44	4.30	6.20+
	0.12	0.24	3.40	14.80+
La	0.10	0.28	4.00	7.90+
	0.12	0.36	2.80	8.00
	0.16	0.28	2.50	5.80+
	0.10	0.32	1.50	8.80+
	0.12	0.28	1.50	6.80+
	0.16	0.36	1.50	6.20
Av.	0.15	1.80	2.65	7.13+
Av. of Av.	0.28	2.76	2.12	11.45+

20 in. per sec. To determine the point on the photograph at which relaxation begins, it is necessary to find the first point where there is a clearly noticeable fall in potentials. This requires a highly sensitive instrument; for the string galvanometer alone,



FIGS. 3–5. RECORDS OF THE COURSE OF RELAXATION

Fig. 3 shows the course of relaxation in the arm flexor muscles of a group of athletes (S) compared with groups of other students not trained to relax (A, A′, B) and with a group of patients (R) previously trained to relax. (On the time-line, abscissa, in Fig. 3 and 4, the point O coincides with the moment of the signal to relax and the first following measurement of V_{2m} is plotted at 0.2 sec. If points closer to O were to be plotted, the curve would of course show that the drop begins not at O, but rather at a somewhat later point before 0.2 sec.)

Fig. 4 shows the course of relaxation in each of 10 athletes.

Fig. 5 illustrates the course of relaxation in the flexor muscle (*Ku*, an athlete not trained to relax). On the time-line (abscissas), the point O is the moment when there occurs the first clearly marked slump in potentials that follows the signal to relax. For the unbroken lines in the graph the determinations were made at the voltage sensitivity 0.3 mm. = 1 ωv.; for the broken lines, 4 mm. = 1 ωv.

as used by Voerckel, is inadequate. It would be arbitrary to formulate a rule for deciding how much of a fall in potentials should be counted as decisive; but in the absence of such a rule there is necessarily an incalculable margin of error. Furthermore, there are instances where it is difficult, if not impossible, to decide whether the first fall observed is or is not merely a fluctuation or inconstancy of contraction.

These various sources of difficulty in determining precisely the beginning point of relaxation perhaps should be considered in accounting for the unexpected finding that for all of the *Ss* in Table I the value of the mean b.rel.t. is lower than that of the mean r.t. While the number of determinations made is too small to warrant generalization, the data indicate that in these *Ss*, following the signal, relaxation frequently is initiated as quickly as—if not even more quickly than—contraction.

Table II presents illustrative data on times of reaction and relaxation. Four of the 10 *Ss* often relax the flexor muscles completely (these are the instances when minimum $V_{2m} = 0$, as set forth in the fourth column); but the other 6 *Ss* consistently fail to relax completely (that is the minimum $V_{2m} > 0$). In 8 of the 10 *Ss*, time of relaxation (*i.e.* to minimum V_{2m}) considerably exceeds time of reaction as a rule. Values twice as large or more are common; and the excess is in some instances as great as fifty- or sixty-fold. In 2 *Ss* who relax completely, reaction-times and complete relaxation-times are approximately equal in a few instances only.

For the athletes as a group, the course of relaxation during the first 5 sec. is plotted in Fig. 3. In this figure are included, for purposes of comparison, curves previously plotted for one group of patients previously trained to relax (R) and for certain groups of untrained 'normal' students (A, A′, B).

As will be noted, the curve for the athletic group falls for the most part between the curves for the 'normal' untrained and for the trained patients. This is evidence that our athletes as a class have a greater ability to relax a part than have the other untrained students; but a lesser ability than have the trained *Ss*. It should be borne in mind that these trained *Ss* suffered originally from certain hypertensive disorders, which doubtless detract from the skill attained in relaxation. An interesting comparison would be between athletes and groups of 'normal' persons trained to relax.

The course of relaxation during the 5-sec. period is shown for each individual athlete in Fig. 4. We shall consider each curve in connection with the comments made by the *S*, which have of course no scientific validity but are quoted for their suggestive interest.

The curve for *Ha* shows very marked failure to relax. He is a general gymnast, age 22 yr., devoted especially to the side-horse. He believes that he characteristically keeps his arms and legs stiffened during exercise on the side-horse in order to maintain proper form and also that before a contest or meet he is perturbed until he gets on the apparatus. He ventures the opinion that like other gymnasts he is not 'good at relaxing.'

The curve for *La* drops to about 4 ωv. at 0.6 sec., but thereafter ranges above that value, showing relaxation markedly incomplete. He is 21 yr. old; plays end in football. He believes that he is somewhat perturbed before a game, but that he is calm after play is well begun.

The curve for *Ku* drops to about 1 ωv. at 0.6 sec., falls further to about 0.5 ωv. at 0.8 sec. then rises, until at 2.0 sec. it reaches 1.5 ωv. and at 3.0 sec. about 3.5 ωv.; thereafter it remains at this or at a somewhat higher level. This *S* plays several positions on the freshman football team, and has practiced fencing for two years previously. He is 18 yr. old. He believes that he does not become perturbed before games. Subsequent curves similarly secured show microvoltage under 1.0 ωv. after

0.6 sec. This individual frequently served in other types of experimental studies thereafter in which he was requested to maintain relaxation of the arm muscles for longer periods of time, wherein he succeeded as a rule, although he showed marked inconsistencies at times. From these determinations it is evident that a curve secured by the methods described in this article has but a highly limited value as representing the ability of an individual to relax a particular muscle-group.

The curve for *Co* drops not quite to 5.0 ωv. at 0.8 sec. and for the remainder of the period lies for the most part between 4.0 and 5.0 ωv. He is 21 yr. old, specializes in tumbling and in exercises on flying rings and parallel bars. He believes that these performances do not conduce to skill in relaxation; for example, rigid control on the parallel bars requires continued contraction. He states that he is nervous before a contest.

The curve for *Cu* at 0.6 sec. has fallen to 1.5 ωv., at 1.0 sec. approximately to 0.5 ωv., then rises until at 2.0 sec. it is nearly 2.0 ωv. and it remains at this level thereafter. This *S*, age 21 yr., plays quarter-back and believes that he is fairly relaxed rather than perturbed throughout the game.

The curve for *Ad* at 0.6 sec. is about 1.5 ωv., at 1.0 sec. and thereafter fluctuates near 1.0 ωv. He is 19 yr. old, a gymnast specializing in exercises on rings, rope climbing, and boxing. He states that he has practiced at relaxing for boxing in order to prevent fatigue and that as a rule he is not nervous at any time.

The curve for *Sm* falls only to 3.0 ωv. at 1.0 sec., falls further to 2.2 ωv. at 2.0 sec. but at 3.0 sec. has risen above 6.0 ωv., whereafter falling until at 5.0 sec. it is about 1.0 ωv. He states that he is 21 yr. old, plays end in football and that he is somewhat 'nervous' before a game, gets tired in the first few minutes of play, but soon recovers and is nervous no longer.

The curve for *Ny* falls to about 1.0 ωv. at 0.6 sec., further to about 0.2 ωv. at 1.0 sec., rising to 2.0 ωv. at 2.0 sec. and remaining above this level until after 4.0 sec., when it falls to about 1.0 ωv. at 5.0 sec. This individual, age 19 yr., plays full-back and between halves is accustomed to close his eyes and relax as well as he can. Before each play, he relaxes as far as possible but becomes tense before the ball is snapped. He does not relax so much on offensive as on defensive play.

The curve for *Sa* drops practically to zero at 0.8 sec. and remains practically at this level until 2.0 sec. A rise occurs toward 3.0 sec., but never as high as 0.5 ωv. At 4.0 sec. the value is zero; at 5.0 sec. slightly above 0.5 ωv. This individual, age 23 yr., plays quarterback, believes that he is not nervous but describes a "hollow feeling in the pit of the stomach" when about to start in offensive tactics.

The curve for *Al* shows a very rapid fall to about 2.3 ωv. in about 0.2 sec.; at 0.6 sec. it is practically at zero but thereafter rises above 1.0 ωv. for an instant. At 1.0 sec. and thereafter the value is indistinguishable from zero. These results are consistent with subsequent studies on this *S*, wherein relaxation in the flexor muscles was almost perfectly maintained for prolonged periods. He is 24 yr. old and often plays quarter-back in practice at football.

In general, the curves indicate that some athletes fail to relax promptly and fail to maintain a state of relaxation. On the other hand, certain other athletes relax almost as well (in this particular test) as do trained *S*s.

It has been traditional to assume that striated muscles fail to relax completely so long as the organism remains in a normal state. But this assumption has rested on no accurate measurements of muscular contraction. Such measurements, as in the present studies, show clearly that zero tonus or complete relaxation can commonly occur in the muscles of normal man, even without training. This leads to the question, how long does it take for a muscle to become completely relaxed?

The period from beginning to complete relaxation in muscle (rel. t. in m.) is found to vary considerably in the individuals tested. Some of the shortest periods observed are illustrated in Fig. 5, compiled from records made with the photographic paper exposed at the rate of about 20 in. per sec. A word of explanation is here necessary. It is not possible with the present apparatus to determine rel. t. in m. from a single photographic record of a test. High voltage sensitivity is required to determine the endpoint, but is not suitable for determining the moment of beginning of muscle rel. t., since the shadow of the recording wire then vibrates to and fro beyond the edges of the photographic record. To overcome this difficulty requires that the apparatus be set for a suitably lower voltage sensitivity. Accordingly, the best means at our disposal to make an approximately correct determination of muscle rel. t. is first to plot the data from a determination made with a relatively lower sensitivity (here 0.3 mm. = 1 ωv.) down to about 3 ωv. and then to add an extension from this curve from data made in the next test with higher sensitivity (here, 4 mm. = 1 ωv.).

In the data employed for this figure, the fall from 3 ωv. to 0 once occurs in 0.05 sec., but in other tests, especially on other individuals, a considerably longer time often elapses—sometimes 0.2 or 0.3 sec. even where the relaxation is fairly progressive. The various curves secured for four athletes as well as for one trained *S* indicate a range for complete ('voluntary') relaxation-time in muscle commonly from about 0.2 to 0.35 sec.

Comment

As previously suggested, the ability of a particular *S* to relax a certain muscle-group is such a variable matter that there is no entirely satisfactory way to determine and formulate it. We have herein determined the time from the signal to relax to beginning relaxation as well as to the point of complete or partial relaxation in a limited number of tests that appear to be fairly characteristic for each individual. Unfortunately it has been impossible to repeat the tests in large numbers as would be required for statistical purposes, since this would develop a practiced skill at the particular test performed. Another difficulty is that to a certain extent *S*s tend to vary from time to time in the speed of reaction and relaxation, depending upon various factors in their personal condition as well as in the laboratory. These considerations lead us to observe caution in drawing conclusions as to the ability of any particular *S* to relax based on this particular test alone; but the test has seemed useful as indicating something of an individual's achievement if considered along with other tests and it has seemed useful especially if applied to groups.

Why is it that athletes as a class evidently excel the average run of university students in ability to relax at will? On general grounds the most plausible reply seems to be that their daily exercises doubtless promote prompt—even if not complete—relaxation, and possibly also that habitual exercise tends to avert unduly prolonged reflection, which, as we have frequently found, is accompanied by persistent slight muscle potentials, *i.e.* failure to relax completely.[10]

Summary and Conclusions

(1) The apparatus available to early investigators was inadequate for the accurate determination of the moment of beginning and of completion of relaxation in muscle.

(2) Contraction in muscles of athletes can be measured in electrical terms (action-potentials) by means of a sufficiently sensitive amplifier-galvanometer system. It is noted that most, if not all, of the amplifier systems heretofore described are not adequate for the determination of slight degrees of contraction or tonus.

(3) In 10 athletes tested, 4 relax the flexor muscles of the forearm completely, sometimes at least, but the other 6 consistently fail to reach this stage. Marked failure to relax is noted in 2 or 3 athletic *S*s.

(4) Beginning relaxation-time in 4 athletes tested is as brief as, or briefer than, reaction-time.

(5) In 8 of the 10 *S*s, relaxation-time (time to least voltage, $V_{2m} \geqq 0$) considerably exceeds reaction-time as a rule. Values twice as large or more are common; and the excess is in some instances as great as fifty- or sixty-fold. In 2 *S*s who relax completely, reaction-times and complete relaxation-times are approximately equal in a few instances only.

(6) As judged by relaxation times, athletes as a group succeed in relaxing a particular muscle-group to a fuller and more sustained extent than do ordinary student *S*s (not trained to relax), but their success is less marked than in *S*s trained to relax, also considered as a group. However, individual athletes can be selected who succeed at this particular test as well as many *S*s trained to relax.

(7) From beginning to completion of relaxation in muscle, under the present conditions, the interval commonly ranges from 0.2 to 0.35 sec. The curve frequently becomes markedly less steep when nearing zero voltage; that is, relaxation often becomes relatively slow in progress as it nears completion.

[10] E. Jacobson, Electrophysiology of mental activities, this JOURNAL, 44, 1932, 677-694.

RIVALRY BETWEEN UNIOCULAR NEGATIVE AFTER-IMAGES AND THE VISION OF THE OTHER EYE

By F. C. SUMNER and F. P. WATTS, Howard University

Parinaud[1] observed in 1882 that, on closing his left eye (L) and fixating a few seconds with his right (R) a red circle lying on a sheet of white paper, and then removing the circle and closing his R eye and then opening his L and maintaining his gaze on the paper as before, "l'image du cercle n'apparaît pas immédiatement. Le blanc du papier s'obscurcit tout d'abord, et c'est seulement alors que l'image se dessine colorée en vert et plus claire que le fond." He gave a cerebral explanation of the phenomenon, *i.e.* that the image thus seen was of central origin.

In 1900 Bocci[2] reported his observation of the phenomenon that occurs on projecting the after-image of a black square surrounded by a white background onto a white or gray field. He likewise believed that the after-image seen by the "resting" eye (*i.e.* the originally unstimulated eye) was of cerebral origin and differed in various respects from the ordinary negative after-image seen by the "active" eye.

The cerebral theory of this phenomenon was justly criticized by several writers[3] on the ground that its proponents failed to exclude the possibility that the after-image belonged to the eye on which the primary stimulus had fallen, and that such an after-image of possibly retinal origin was either combining or entering into rivalry with the field presented to the other (open) eye. Thus far work on the phenomenon was solely for the purpose of supporting the one or the other of these two theories and the outcome of this controversy was a gradual augmentation of evidence in favor of the rivalry (retinal fag) theory.

In 1930 two British physiologists, Creed and Harding,[4] working in a dark room and using white circular disks as fixation objects and white or black projection fields for the other eye, were the first experimenters in this field to study, even in a somewhat limited fashion, the dependency of this phenomenon and its latent period upon stimulus-variables. They report the non-occurrence of the phenomenon with 15-sec. fixations and its occurrence with 30-sec. fixations; that the image of this phenomenon resembles in its diminished intensity the ordinary uniocular negative after-image; "that when a uniocular black projection after-image is in rivalry with a white field presented to the other eye, it appears earlier than when, in the absence of any stimulus to the other retina (black projection field), it might be expected to develop with less hindrance."

* Accepted for publication September 30, 1935.

[1] H. Parinaud, Du siège cérébral des images accidentelles ou consécutives, *Gazette des Hôpitaux*, 55, 1882, 459 f.

[2] B. Bocci, *Note ed appunti di tecnica fisiologica*, 1900, 11-23.

[3] E. B. Delabarre, On the seat of optical after-images, this JOURNAL, 2, 1889, 326-328; H. Ebbinghaus, Über Nachbilder in binocularen Sehen und die binocularen Farbenerscheinungen überhaupt, Pflüger's *Arch. f. d. ges. Physiol.*, 46, 1890, 498-508; W. McDougall, The sensations excited by a single momentary stimulation of the eye, *Brit. J. Psychol.*, 1, 1904, 98; J. von Kries, in H. von Helmholtz, *Handbuch der physiologischen Optik*, ed., 3, 1910, 431.

[4] R. S. Creed and R. D. Harding, Latency of after-images and interaction between the two retino-cerebral apparatuses in man, *J. Physiol.*, 69, 1930, 423-441.

Assuming that the phenomenon is due in large part to retinal rivalry (retinal fag) and to fluctuation of attention, we have proposed in the present experiment to make a more detailed phenomenological study of this sensory event with a view to ascertaining on what stimulus-variables the phenomenon depends.

Procedure. The procedure was divided into Parts I and II. In Part I, the observations were made in clear daylight between 10 A.M. and 3 P.M. The objects to be

TABLE I

DAYLIGHT EXPERIMENT, SHOWING FOR EVERY FIXATION-COLOR, FOR EACH EYE-PROCEDURE, AND FOR EVERY PROJECTION BACKGROUND, THE LATENT PERIOD AND DURATION OF THE PHENOMENON IN SEC.

(Fixation-background: neutral gray. Fixation time: 30 sec.)

Fixation-color	Projection-background	Eye-Procedure (R→L)				Eye-Procedure (L→R)			
		Latent period		Duration of phenomenon		Latent period		Duration of phenomenon	
		Su	*Wa*	*Su*	*Wa*	*Su*	*Wa*	*Su*	*Wa*
red	white	19.0	10.8	3.2	0.7	9.0	7.2	3.0	1.8
	gray	13.8	6.0	3.0	4.8	11.4	17.4	5.4	1.8
	black	7.8	10.8	3.6	1.2	6.0	—	1.8	—
green	white	—	—	—	—	15.6	—	1.1	—
	gray	—	—	—	—	16.8	—	1.2	—
	black	—	—	—	—	17.4	—	1.8	—
blue	white	12.6	9.0	1.4	.2	10.4	13.8	3.0	1.1
	gray	14.4	8.0	1.2	3.0	5.4	17.4	2.4	2.4
	black	4.2	15.3	3.6	1.8	5.4	—	3.7	—
yellow	white	—	—	—	—	10.4	—	2.6	—
	gray	—	10.8	—	4.2	—	—	—	—
	black	—	—	—	—	—	—	—	—
black	white	10.8	13.2	3.2	3.9	12.3	15.6	2.7	3.3
	gray	6.0	11.4	3.6	2.4	6.6	—	1.2	—
	black	5.0	12.6	3.6	1.8	8.4	18.0	4.8	3.6
white	white	—	—	—	—	16.8	—	1.1	—
	gray	—	—	—	—	—	—	—	—
	black	—	—	—	—	—	—	—	—

fixated by *O*'s unshielded eye were 1½-in. squares of white, black, red, green, blue, and yellow paper (corresponding to the primary colors of the Hering color circle) pasted on black, white, and neutral gray backgrounds of cardboard (6 x 7 in.) of lusterless surface. *O* fixated the object from a distance of 2 ft. for 30 sec. in all but one experimental series. This latter series was conducted to see what effect a longer fixation-period, one of 60 sec. would have on the phenomenon. At the expiration of fixation-periods, *E* covered the fixation-card with the projection-card of similar size,—white, gray, or black as the experimental series required. Meanwhile *O* closed the unshielded eye and fixated with the previously shielded one the center of the projection-field. Time was measured with three stop-watches. *E* used one watch

for measuring the exposure or fixation-period and another watch for measuring the latency-period; and *O* used the third stop-watch to measure the duration of the phenomenon. The phenomenon was considered as not occurring when three trials failed to yield it. Introspections were taken immediately following the disappearance of the phenomenon and prior to recording time.

In Part II the procedure was practically the same as in Part I with the exception that the experiment was performed in a dark room equipped with a Corning daylight lamp.

The *Os* throughout the entire experimentation were Sumner (*Su*) and Watts (*Wa*), who alternated as *O* and *E*.

Results. The findings are presented in Tables I–IV. Interpretations of these tables and phenomenological data will be found in the next section.

TABLE II

DAYLIGHT EXPERIMENT, SHOWING FOR EVERY FIXATION-COLOR THE LATENT PERIOD AND DURATION OF THE PHENOMENON IN SEC. WHEN EYE-PROCEDURE, FIXATION-BACKGROUND AND PROJECTION BACKGROUND ARE CONSTANT AND FIXATION PERIOD IS LENGTHENED

(Fixation background: neutral gray. Fixation time: 60 sec. Eye procedure: R→L. Projection background: white.)

Fixation-color	Eye-procedure (R→L)			
	Latent period		Duration of phenomenon	
	Su	*Wa*	*Su*	*Wa*
red	7.8	7.8	4.8	2.2
green	—	—	—	—
blue	13.8	9.6	2.7	2.4
yellow	15.0	8.4	3.6	6.1
black	4.8	12.0	2.8	4.4
white	—	33.0	—	1.6

Discussion. (1) A phenomenological description. The phenomenon was considered as not occurring when in three trials the projection-field of the previously shielded eye appeared to remain unchanged for a period of 45 sec. Three degrees of its occurrence can be distinguished:

(a) In the first degree the projection-field of the previously shielded eye becomes gradually dark, conspicuously so with a white or gray projection-field, and, when in the center of this darkened projection-field, appears a more or less sharply outlined figure of about 1 in. sq. having the color complementary to the previously fixated object. With Red, Blue, or Black fixation-colors the phenomenon regularly occurs in the first degree.

(b) In the second degree the projection-field of the previously shielded eye becomes gradually dark after some seconds and the center has a color complementary to that of the previously fixated square; but the outline of this colored area is vague and irregular or shades off into the darkened background, thus resembling more a 'patch' of color.

(c) In the third degree the phenomenon is evidenced only by the darkening of the projection-field with no color or merely a small white or colorless patch in center.

In the rare cases in which the phenomenon has occurred with White, Yellow, or Green fixation-objects, it has been either of the second or third degree save in the 60-sec.-fixation experiment (Table II) and the experiment in which fixation-backgrounds were black and projection-fields white (Table III). In these exempted series White, Yellow, and Green fixation-objects yielded the phenomenon in the first degree. In none of the cases of White, Yellow, and Green fixation-objects reported in Table I was the phenomenon yielded in other than the third degree.

TABLE III

DAYLIGHT EXPERIMENT, SHOWING FOR EVERY FIXATION-COLOR AND FOR EACH EYE-PROCEDURE THE LATENT PERIOD AND DURATION OF THE PHENOMENON IN SEC. WHEN FIXATION BACKGROUND IS BLACK AND PROJECTION BACKGROUND WHITE AND THE REVERSE

(Fixation time: 30 sec.)

Fixation-color	Background of		Eye-procedure (R→L)				Eye-procedure (L→R)			
			Latent period		Duration of phenomenon		Latent period		Duration of phenomenon	
	Fixation	Projection	*Su*	*Wa*	*Su*	*Wa*	*Su*	*Wa*	*Su*	*Wa*
red	black	white	8.1	9.6	2.4	2.4	9.9	17.4	1.2	1.2
	white	black	6.6	11.1	3.0	0.9	8.4	15.6	2.4	0.5
green	black	white	9.6	11.4	3.0	1.2	10.0	12.3	1.0	1.0
	white	black	—	—	—	—	—	—	—	—
blue	black	white	13.8	11.4	2.5	3.0	14.4	19.2	2.4	1.0
	white	black	12.9	17.4	2.0	1.2	13.5	21.0	3.0	1.0
yellow	black	white	8.4	8.4	2.4	4.2	14.4	28.8	0.6	1.0
	white	black	—	9.9	—	0.6	12.0	—	0.6	—
white	black	white	11.4	8.4	2.7	1.2	9.0	19.2	2.4	1.0
black	white	black	8.4	13.2	3.0	3.0	7.5	25.5	4.8	0.5

(2) Intensity of the negative after-image in this phenomenon. The after-images involved in this phenomenon are noticeably fainter than ordinary uniocular and binocular after-images. The prolonged latency of the phenomenon coupled with the onset of fading in the negative after-image of the originally stimulated eye undoubtedly accounts for this diminution of intensity.

(3) Size of the negative after-image in this phenomenon. In cases of Black, Red, and Blue fixation-objects, it was noted that the size of this rivalry-after-image is appreciably smaller than that of the originally fixated square, and also smaller than that of the ordinary uniocular or binocular negative after-image. This noticeable decrease in size may be explained

by the fact that the negative after-image of the originally stimulated eye is in reality seen projected in the rivalry-phenomenon against the darkened inside of the closed eyelid rather than against a screen two feet distant.

(4) Latency-period of the phenomenon. A rough inspection of the latent periods of the phenomenon reveals that an appreciable time is necessary in all successful cases of the occurrence of the phenomenon—a period

TABLE IV

DARK-ROOM EXPERIMENT, SHOWING FOR EVERY FIXATION-COLOR THE LATENT PERIOD AND DURATION OF THE PHENOMENON IN SEC. WHEN FIXATION BACKGROUND AND EYE-PROCEDURE ARE CONSTANT

(Fixation background: neutral gray. Eye-procedure: R→L. Fixation time: 30 sec.)

Fixation-color	Projection-background	Eye-procedure (R→L)			
		Latent period		Duration of phenomenon	
		Su	*Wa*	*Su*	*Wa*
red	black	5.0	21.0	2.4	.5
	white	3.6	9.6	1.8	2.4
green	black	—	—	—	—
	white	—	—	—	—
blue	black	10.5	14.4	1.8	1.5
	white	3.6	5.4	2.4	6.0
yellow	black	—	—	—	—
	white	12.9	11.4	2.4	1.0
black	black	5.0	14.4	1.8	1.0
	white	4.8	24.0	2.4	.8
white	black	—	—	—	—
	white	—	—	—	—

ranging from 3.6 sec. to 33.0 sec. with the average period about 10 sec. The extreme variability of the latency periods would make it seem inadvisable to draw too precise conclusions on their basis without obtaining a large number of measurements of a comparable character. Relative attention-values and retinal fatigue both account for the generally long latent period of this phenomenon.

(5) Duration of the phenomenon. The minimum duration was 0.22 sec. while the maximum duration was 6.07 sec. Average durations figured from Table I are as follows:

R→L *Su* 2.92	L→R *Su* 2.56
R→L *Wa* 2.39	L→R *Wa* 2.34

It appears that, the longer the latency period, the briefer the duration of the phenomenon, and that there is a certain amount of individual difference in duration. *Wa* has a shorter duration than *Su* whereas the latent periods were consistently longer for *Wa*.

(6) The darkening of the projection-field. The projection-field darkened whether or not the negative after-image of the small colored square was present. This darkening effect is more pronounced, the lighter the projection field, and it may be attributed to the darkness of the negative after-image of the fixation-background when the latter is white or gray and to contrast-effect of this dark negative after-image with white projection-field.

Both the darkening of the projection-field and also the negative after-image of the little colored square appear as a veil or cloud against the projection-field. A tilting of the *O*'s head to one side during the phenomenon will tilt this whole veil-like phenomenon in a catercornered fashion with respect to the projection-background.

(7) Recurrence of the phenomenon. In pronounced cases of this rivalry-phenomenon, as with Red, Blue, and Black fixation-objects, there was observed not only a prolongation of the phenomenon but also a recurrence of it after a few seconds.

(8) Comparative influence of varied colored fixation-objects. The most striking result of this experimentation is the fact that all six fixation-colors do not equally yield the phenomenon under discussion. Red, Blue, and Black fixation-objects yielded it most vividly and in sharpest outlines in nearly every experimental series, whether the fixation-period was 30 sec. or 60 sec., whether the fixation-background was White, Gray, or Black, whether the projection-field was White, Gray, or Black, whether the experiment was conducted in actual daylight or in a dark room, or whether the procedure was R→L or L→R. On the other hand Green, Yellow, and White fixation-objects conspicuously failed to produce the phenomenon except when the fixation-period was lengthened to 60 sec. or when with 30-sec. fixation the fixation-background was Black and the projection-field was White. Apparently all six colors can be made under certain conditions to yield the phenomenon but those conditions have to be of a special sort for Green, Yellow, and White. Among such special conditions may be included an intensification of the light-source of the fixation-color, the making of the fixation-background black and the projection-field white, and a prolongation of the fixation period to 60 sec. or more.

Parinaud observed the phenomenon after fixating a red circle, Bocci after fixating a black square. Both these fixation-colors in our experiment yielded regularly the

phenomenon. Our results make us question, however, the apparent ease with which Creed and Harding obtained the phenomenon with 30-sec. fixations of a white disk, in as much as we failed so frequently to obtain the phenomenon with a 30-sec. fixation of white either with light or dark adaptation.

The non-occurrence of the phenomenon following 30-sec. fixations of Green, Yellow, and White objects is obviously due to (1) too short a uniocular fixation of these colors to give an intense enough negative after-image to rival successfully the projection-field of the other eye, except where the fixation-background is black and the projection field is white; (2) the greater indistinguishableness of the uniocular negative after-image of Green, Yellow, and White objects from the dark background furnished

TABLE V

COMPARISON OF THE LATENT PERIODS FOR BLACK AND WHITE BACKGROUNDS

Fixation-color	Projection background			
	Black		White	
	Latent period		Latent period	
	Su	*Wa*	*Su*	*Wa*
red	5.0	21.0	3.6	9.6
blue	10.5	14.4	3.6	5.4
black	5.0	14.4	4.8	24.0
	20.5	49.8	12.0	39.0
Av. latent periods	6.83	16.60	4.00	13.00

by the closed and shielded eyelid; and (3) the lesser relative attention-value of the black, purplish red, and purplish blue of the negative after-image than of the white, black, and neutral gray sensations from the projection-fields of the other eye.

(9) Influence of the projection-field on the phenomenon. The rivalry of the uniocular negative after-image with the projection-field of the other unstimulated eye is more conspicuously observed with a white projection-field than with a gray or black one, and fixation-colors which do not with 30-sec. fixation produce the rivalry-effect such as White, Yellow, and Green when the projection-field is black or gray, do so more regularly when the projection-field is white. Latent periods appear more frequently shorter for white projection-fields than for black when the experiment is conducted under dark adaptation. The figures of Table V are taken from Table IV (Dark-room Experiment) where the constants are dark adaptation, gray fixation-backgrounds, 30-sec. fixations and R→L procedure.

(10) Influence of personal equation. With respect to the occurrence

of the phenomenon there was practically no difference between the two *Os*. On the other hand, individual differences in latent periods and duration of the phenomenon appear to exist between the two *Os* in this experiment.

Summary. The results of this study may be summarized as follows:

(1) The successful rivalry of a uniocular negative after-image with the projection field of the other, originally unstimulated eye depends intimately on the color of the original fixation-object, the length of the fixation-period, the fixation-background and the projection-field.

(2) The phenomenon is yielded by all fixation colors when the fixation background is black and the projection field is white or when the fixation period is 60 sec.

(3) The longer the latent period, the shorter the duration of the phenomenon.

(4) The negative after-image involved in this phenomenon is slightly smaller and fainter than the ordinary uniocular and binocular negative after-image.

THE ADAPTATION OF AREAL PAIN

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In an earlier report we presented the results of a study on the adaptation of pain aroused by heat,[1] in which we used an areal stimulus (a small spot of radiant heat) 2 mm. in diam. This particular size was adopted after preliminary trials in which we found that smaller diameters did not permit sufficient heat to pass through the opening of the stimulator to arouse pain within the time-limits at our disposal, while larger diameters gave areal pains which failed to adapt completely even after 45 min. of stimulation.[2] Since punctiform stimuli alone were used in earlier studies on the adaptation of pain,[3] there was nothing in the literature to guide us in the interpretation of the non-adapting areal pains. There are, however, three possible explanations.

(1) The failure of the larger areas to come to complete adaptation may be due sheerly to *size*. Areal pains larger than a certain minimum may be intrinsically non-adaptable.

(2) It may be due to the *progressive nature of the stimulus*—the explanation that we favored.[4] Radiant heat penetrates deeper and deeper into the tissues of the skin with time and, as it progresses inward, it may stimulate the underlying pain-receptors before the more superficial receptors are completely adapted. Accordingly, when small areas (*e.g.* those about 2 mm. in diam.) and relatively few receptors are involved, the penetration may not proceed at a rate sufficiently rapid to forestall

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[1] L. J. Stone and K. M. Dallenbach, Adaptation to the pain of radiant heat, this JOURNAL, 46, 1934, 229-242.

[2] *Op. cit.*, 232, footnote 16.

[3] Max von Frey, Beiträge zur Physiologie der Schmerzsinnes, *Ber. ü. d. Verhandl. d. konigl. sächs. Ges. d. Wiss. z. Leipzig*, math.-phys. Kl., 46, 1894, 193; *Untersuchungen über die Sinnesfunktionen der menschlichen Haut*, 1896, 242; Versuch über schmerzerregende Reize, *Zsch. f. Biol.*, 76, 1922, 9.

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[4] *Op. cit.*, 239.

complete adaptation. On the other hand, when large areas and numerous receptors are involved, the chance of obtaining complete adaptation of all the receptors at one level, before others at a lower level are stimulated, is greatly decreased; and the larger the stimulus-area employed the more remote is that probability. If this be the explanation of our failure with large areas, then stimulus-size is only a secondary factor and an areal stimulus that is stable and constant in its effect should yield complete adaptation.

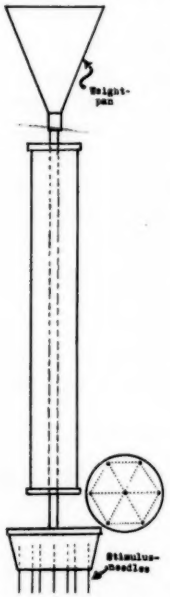

FIG. 1. DIAGRAM OF ALGOMETER
Insert showing arrangement of stimulus-needles.

(3) Our failure may be due to the fact that the *multiple pains* aroused in the large areas are inherently non-adaptable. (Our *O*s often reported numerous punctiform spots of pain in the area under observation, that varied independently both in intensity and in spatial character.) If this be the case, then stimulus-size is, again, only secondarily involved and a many-pointed stimulus that is constant and stable in its effect should *not* lead to complete adaptation.

The present study was undertaken in order to determine which of these explanations was correct. If large areal pains or multiple pains are not adaptable under constant conditions, then the question of pain-adaptation raised by the Von Frey-Goldscheider controversy[5] will again be placed upon the experimental carpet. If both types of pain prove to be completely adaptable, then the explanation favored in our earlier study will be confirmed, and the results with areal and many-pointed stimulation will fall into line with those with single punctiform stimuli.

We wished specifically to discover whether areal and many pointed pain-stimuli, of diameters greater than 2 mm., would come to complete adaptation when they were stable and constant in their effect.

METHOD AND PROCEDURE

We chose for our investigation (1) the largest flat area (2) on the smallest part of the dorsal surface of the forearms of our (3) smallest *O*. By doing this, (1) we avoided the complications that arm-curvature entails in applying the stimulus at a uniform and constant pressure; (2) we were able to use the forearm along its entire length without changing the size of the stimulator; and (3) we could prepare one stimulus-object and use it with all our *O*s.

The area meeting these specifications was about 20 mm. in diam. So as to be well within this limit and to provide us with a comfortable margin of safety, we made our stimulus-object 15 mm. in diam. We intended to bracket to smaller areas if and when occasion demanded.

Apparatus. The stimulus-object was a many-pointed algometer (Fig. 1) contain-

[5] For a discussion and references cf. Stone and Dallenbach, *op. cit.*, 229.

ing 7 small steel needles (size no. 10) which were set, as shown in the insert in Fig. 1, at the angles and center of a 15 mm. hexagon. The algometer was held in a universal standard and the stimulus-points were brought to bear upon *O*'s forearm by means of a rack-and-pinion gear. Since the apparatus operated upon the all-or-none principle, the stimulus was applied at once and to its full intensity. Six different stimulus-intensities were used: 28.5, 33.5, 43.5, 73.5, 103.5, 153.5 grm.,[6] but not with all the *O*s. Table II below records the particular weights and the number of different ones used with the different *O*s. We introduced this variation to take care of individual differences and partly for experimental purposes. The *O*s' fore-arms were held rigidly in place by means of individual plaster of Paris casts.

Observers. The *O*s were Dr. A. D. Glanville (*G*), research fellow in psychology; Mr. W. S. Neff (*N*) and Mr. J. W. Macmillan (*M*), graduate students in psychology; and Mr. R. Fleischman (*F*) and Mr. H. A. Sklarsky (*S*) undergraduates. Since all had served as *O*s in the experiment on the adaptation to the pain of radiant heat,[7] they came to the present study highly trained in the observation and report of pain-adaptation.

Instructions. The instructions, identical with those given in the earlier study and read to the *O*s at the beginning of every experimental hour, were as follows:

> At a 'Ready,' 'Now' signal, your arm will be stimulated. Describe the temporal course of your experience. Give particular emphasis to the report of the appearance and disappearance of pain. Relax and remain as motionless as possible during the course of the experiment. Close your eyes at the signal, and do not open them until told to do so.

Procedure. The general procedure was the same as in our earlier study.[8] *O* seated himself in a comfortable chair at a table and placed one of his arms into a plaster cast. A screen obstructed *O*'s view of his arm. Before every experiment, the cast was adjusted so that the surface to be stimulated was perfectly horizontal. At 'Ready' the algometer was slowly lowered to the surface of *O*'s arm, and at 'Now' the stimulus-weight was transferred to his skin.

As in our earlier study, *O*'s arms were used alternately. The stimulus-area on each arm was divided into quarters, and numbered 1 to 4 from the elbow to the wrist.

Stimulation was continued until 20 sec. after *O* had reported the disappearance of pain without reoccurrence—the criterion of complete adaptation employed in the earlier studies.[9]

O's reports—running descriptions of his experiences—were given orally and recorded by *E*, together with the time of stimulation, from a stop watch.

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[6] These values, determined by means of a chemical balance, include the weight of the stimulus-point, connecting rod, weight-pan (all three of which together weighed 23.5 grm.) and the weights placed in the pan. We were greatly surprised at the amount of weight required to arouse pain. We expected a sevenfold increase over the values used by Burns and Dallenbach (*op. cit.*, 112) since we were using seven points instead of one; but a sevenfold increase of their minimal value merely yielded pressure. Though no effort was made to determine the pain-limen with a 7-pointed stimulus-object, we discovered empirically that we had to increase Burns and Dallenbach's weights from 11- to 17-fold to arouse pain in our *O*s. If the needles were in close juxtaposition, the wedging and mutual interference which they would exert upon one another could explain this increment. But our needles had a separation of 7.5 mm.!

[7] *Op. cit.*, 233.

[8] *Op. cit.*, 233.

[9] Cf. Burns and Dallenbach, *op. cit.*, 112, 115; Stone and Dallenbach, *op. cit.*, 233, 238.

After a short practice series, 25 experiments were performed with every one of the 5 *Os*.

Results. (1) Spatial character. Both areal and punctiform pains were aroused by our stimulus. All the *Os*, with the exception of *S* who did not describe the spatial character of his experiences, reported both types. Our *Os*, it should be recalled, had not been instructed to report upon the spatial aspect of their experiences. As shown in Table I, *G* reported upon this aspect in all the experiments, *M* and *N* in more than half, and *F* in about a third. *G*, who reported 24 punctiform pains to 1 areal, was disposed to the punctiform. *N* (5 punctiform and 15 areal) was disposed to the areal.

TABLE I

SPATIAL CHARACTER OF THE PAINS

(25 experiments for every *O*)

O	Not reported	Areal	Punctiform
F	17	4	4
G	0	1	24
*M**	11	8	7
N	5	15	5
S	25	0	0

* M's total is 26 because one experiment was counted twice (see text.)

Areal disposition may also be attributed to *M* and *F* (8 areal to 7 punctiform and 4 to 4), since both indicated that they had taken areal pains of the size of the algometer for granted, and so failed to report upon their areal character. Most frequently the areal pains approximated the size of the stimulator, but they ranged in size from small areas "of the size of a pea" (*M*, 43.5 grm.) to large and diffuse areas involving a considerable part of the forearm; *e.g. N*'s report of a "sharp pain, very bright; whole area extending toward elbow painful; pain rather intense and uniform" (area 2, the second quarter of forearm from elbow, stimulated with 103.5 grm.).

The punctiform pains were "sharp and pointed like a needle" (*F*, stimulus-weight 33.5 grm.). They varied in number from one—the most frequently reported—to three (*G*). There were also reports of "two or three sharp pains" (*G*, 33.5 grm.) and "sharp pains in a number of spots" (*G*, 43.5 grm.). The correspondence to the 7-pointed physical stimulus—of which the *Os* had no knowledge—was at no time close.

At times the punctiform character changed into areal, *e.g.* "sharp pain, punctiform at first, now it is areal" (*N* 43.5 grm.). At other times, but with less frequency, the areal pain turned punctiform, *e.g.* "a small area at

first, now a single point of pain" (*M*, 43.5 grm.). Such reports were tabulated according to the first characterization given. One report by *M* read "pain is areal, but one point is sharper." This case was recorded in Table I as both areal and punctiform—hence the number of cases for *M* there totals 26.

Spatial character was independent, as far as we could discover, of any of

TABLE II

AVERAGE ADAPTATION-TIME (IN SEC.), M.V., AND RANGE OF THE DISPERSION

(25 experiments for every *O*)

O	Stimulus-weight (grm.)	No. of Exp.	Adaptation-time in sec.			m.v./av.
			av.	m.v.	range	
F	73.5	4	298.3	218.7	43–672	73%
	43.5	8	244.0	193.2	9–755	79%
	33.5*	12	161.4	72.2	13–283	45%
G	153.5	2	42.5	17.5	25– 60	41%
	43.5	15	136.9	77.5	20–361	57%
	33.5	8	99.6	44.3	28–182	44%
M	43.5	25	56.6	30.8	15–138	54%
N	153.5	1	295.9	—	—	—
	103.5	3	452.7	111.6	361–620	25%
	43.5	5	355.4	63.8	229–476	18%
	33.5	8	128.0	87.3	30–437	68%
	28.5	8	52.9	21.7	11–117	41%
S	73.5	11	113.0	66.7	14–261	59%
	43.5	14	72.0	79.3	5–288	110%

* One experiment with this weight failed to yield adaptation.

the variable moments in our procedure, such as stimulus-weight or stimulated region.

The fact that both spatial types—areal and punctiform—were obtained from our algometer was fortunate. It enabled us to test in this single series of experiments all three of the possible causes of our earlier failure to obtain complete adaptation from large stimulus-areas. If complete adaptation had occurred in none of the trials, then we should have evidence that large areal pains are non-adaptable. If it were obtained only in the experiments yielding areal and not in those yielding punctiform pains, then we should be justified in concluding that multiple pains are non-adaptable. If, however, complete adaptation were obtained with both areal and punctiform pains, then we should be justified in our early explanation and should feel warranted in reaffirming it.

(2) Adaptation. Complete adaptation, as defined in terms of our criterion, occurred in *all but one* of our experiments, *i.e.* in 124 of the 125 trials.[10] The adaptation-time, as shown in Table II, varied with the *O* (9–755 sec. for *F*, 20–361 sec. for *G*, 15–138 sec. for *M*, 11–620 sec. for *N*, and 5–288 sec. for S) ; with intensity of stimulus (229–620 sec. for 103.5 grm., 14–672 sec. for 73.5 grm., 5–755 sec. for 43.5 grm., 13–437 sec. for 33.5 grm., and 11–117 sec. for 28.5 grm.) ; and in different trials with the same *O* and the same intensity—*e.g.* *F* (43.5 grm.) 9–755 sec., *S* (73.5 grm.) 14–261 sec., *N* (33.5 grm.) 30–437 sec.

The average adaptation-times of the areal and punctiform pains are given in Table III. *F* and *N* required on an average nearly twice as much

TABLE III

ADAPTATION-TIME (IN SEC.) OF AREAL AND PUNCTIFORM PAIN

O	Areal				Punctiform			
	No.	Time	m.v.	m.v./av.	No.	Time	m.v.	m.v./av.
F	3*	459.0	197.3	43%	4	254.5	53.0	21%
G	1	200.0	—	—	24	113.5	82.0	72%
M†	7	37.1	12.7	34%	6	85.6	37.9	44%
N	15	246.0	119.2	48%	5	141.4	133.8	94%

* Not including one instance of areal pain that failed to adapt.

† Not including the instance of areal-punctiform pain. This came to complete adaptation n 45 sec.

time for the areal pains (459 and 246 sec.) as for the punctiform (254.5 and 141.4 sec.). *M*, on the contrary, required more than twice as much time for the punctiform pains (85.6 sec.) as for the areal (37.1 sec.). While the differences between the averages for areal and punctiform pain are large, (+204.5 sec. for *F*, +104.6 sec. for *N*, and —48.5 sec. for *M*), we believe that no generalized statement is warranted. Not only do the differences lie in opposite directions, but the cases are few in number (3 to 15) and the deviations from the averages are large (21% to 94%). If there is any difference in adaptation-time between areal and punctiform pain, it is individual.

There seems to be a slight tendency, however, for adaptation-time to increase with intensity of stimulus. As shown in Table II above, *F*'s average times varied from 161.4 sec. at 33.5 grm. to 298.3 sec. at 73.5 grm.; *G*'s, from 99.6 sec. at 33.5 grm. to 136.9 sec. at 43.5 grm.; *N*'s, from 21.7 sec.

[10] The exception occurred in the middle of *F*'s experimental series (trial 18) and involved the lightest intensity of stimulus (33.5 grm.) used with him. Had our criterion of adaptation been less rigorous, we should not have had this exception, for during the course of this experiment, which was prolonged 25 min., the pain momentarily disappeared several times.

at 28.5 grm. to 111.6 sec. at 103.5 grm.; and *S*'s, from 72.0 sec. at 43.5 grm. to 113.0 sec. at 73.5 grm. Again, however, the variations from the averages and the ranges of dispersion are so large and the trials with different stimulus-intensities are so few in number that little, if any, significance can be attached to the results.

Still, the differences cannot be accounted for by fatigue or ennui, for variations in adaptation-time were as marked during the first as during the last part of the experimental hour, and during the early as during the later part of the study. The results are similar in the main to those obtained by Burns and Dallenbach.[11] If adaptation-time is conditioned by stimulus-intensity, the correlation is obscured by differences in sensitivity or tuning of the spots that are stimulated by chance in the haphazard placement of the algometer.[12]

(3) Normal course. The normal and typical course of the experience aroused by the many-pointed stimulation of the skin is illustrated by the following reports:

F: [Exp. 8, 43.5 grm., left arm, area 1]: (0″) pressure and then almost immediately a very slight pain; (17″) pain slightly stronger; (27″) more intense pain, pressure less; (1′19″) pain growing less intense; (1′24″) a warm feeling there now, only slightly painful; (1′57″) pain disappeared, only pressure and warmth left; (2′17″, when stimulus was removed) twinge of pain.[13]

G: [Exp. 5, 43.5 grm., right arm, area 3]: (0″) sharp pain; (4″) fading in intensity; (8″) circular area of pressure with pain within it; (23″) only two or three points of pain now; (49″) very weak pain; (54″) more intense pressure around the pain—rest of pressure is very weak; (1′7″) one point of very weak pain; (1′12″) that point is no longer pain but sharp pressure; (1′32″, when stimulus was removed) pain.

M: [Exp. 2, 43.5 grm., right arm, area 3]: (0″) pressure, followed almost immediately by pain; (7″) now very slight pain; (13″) pain a little more intense; (18″) pain getting less intense; (26″) very slight pain; (35″) only slight itch and pressure now; (47″) just pressure; (1′5″) dull pressure; (1′7″, when stimulus was removed) sharp flash of pain.

N: [Exp. 5, 43.5 grm., right arm, area 1]: (0″) contact and areal pain; (10″) complex of cold, punctiform pain and vague contact; (40″) same; (1′2″) pain decreasing in size—is not sharply localized in one place; (1′20″) it is oscillating now; (1′33″) very thin small point of pain, quite sharp; (1′45″) fluctuates in intensity; (2′7″) sharp pain; (2′30″) sharp pain; (2′40″) still sharp pain; (2′57″) sharp and thin pain; (3′27″) same; (3′52″) same; (4′11″) decreasing in intensity, cold sharp point; (4′20″) sharp pain waxes and wanes; (4′55″) cold with sharp pain; (4′58″) ache; (5′) dull pain; (5′30″) thrill of pain a couple of times;

[11] *Op. cit.*, 113 f.

[12] Until some means is discovered of stimulating identical spots in the skin at different times and with different intensities of stimulus, the correlation must perforce remain obscure. Multiplication of experiments in which the placement of the stimulus-point is left to chance will not suffice—unless the sampling is extremely large—because of variations in sensitivity and tuning.

[13] The numbers in parentheses in this and in the following reports show the time that elapsed from the beginning of the experiment to the giving of the report.

(5′52″) no pain, constricted, drawn, pressury area; (6′12″, when stimulus was removed) pain.

S: [Exp. 6, 73.5 grm., left arm, area 2]: (0″) pressure and pain; (14″) pain less intense; (17″) pressure less intense; (31″) pain very slight; (43″) pressure very slight; (49″) pain slight, pressure very slight; (1′40″) no pain; (1′45″) pressure less; (1′47″) pressure little greater; (2′, when stimulus was removed) slight pain.

[Exp. 24, 43.5 grm., right arm, area 1]: (0″) pressure and pain; (5″) pain gone; (12″) slight pressure; (23″) pressure is now very slight; (25″, when stimulus was removed) slight stab of pain.

In general, the first experiences aroused were pressury or painful in quality. Pain appeared either before, or simultaneously with, or immediately after pressure.[14] The qualities endured together for a time, waxing and waning in intensity; then pain declined and disappeared, leaving pressure. When the algometer was removed (20 sec. after the report of the disappearance of pain), pain reappeared.

The persistence of pressure in these experiments indicates, as do the results of earlier studies on the adaptation of punctiform pressure-pain stimuli, that, given favorable conditions, pressure is less readily adapted than pain. There is nothing in these results, as Burns and Dallenbach pointed out,[15] to justify Goldscheider's contention that pain is a summation of pressure. We used a pressure-pain stimulus that was adequate to both of those qualities. These results show that the residual effects are the same for areal as for punctiform stimuli.

(4) After-pain. Some quality of pain (itch, prick, sharp or bright pain) reëmerged in every experiment at the removal of the algometer. The after-pains were usually of short duration, but at times they persisted for several minutes. The *O*s did not state whether these after-pains were areal or punctiform, and we did not press them for descriptions. We merely note, therefore, the occurrence of after-pains, and the correspondence between our results with a many-pointed stimulator and the results obtained by Burns and Dallenbach with punctiform stimuli.[16]

Summary and Conclusions

Areal and multiple pains, aroused by a constant and unvarying stimulus 15 mm. in diam., adapted completely for all of our *O*s. This result gives us the answer to our problem. Since both types of pain are completely adaptable and since our stimulus was stable, constant, and unvarying, the explanation suggested in our earlier study is confirmed. Our failure in

[14] Our stimuli were applied at superliminal pressure values. We should, therefore, expect that pressure would be aroused and furthermore, since pressure has a shorter latent period than pain, that it would normally be aroused first.

[15] *Op. cit.*, 115 f. [16] *Ibid.*, 116 f.

that study to obtain complete adaptation in the case of areas larger than 2 mm. in diam. was due to the continuous and progressive effect of the stimulus. Adaptation did not proceed as rapidly in the superficial receptors as the radiation from the heat-stimulator penetrated the tissues and stimulated new endorgans. We have no need, therefore, of qualifying any of the conclusions based upon the results of our early study.

Except for differences in absolute adaptation-time, the results of our study with areal and many-pointed stimuli agree closely with those obtained by Burns and Dallenbach with punctiform stimuli. Our adaptation-times were much longer than theirs,[17] as is to be expected. Otherwise, the differences are negligible. Both studies show that (1) adaptation-time varies with the *O* and with the area or spot stimulated and that the percentages of variation are comparable in magnitude; (2) that adaptation-time is not compulsorily conditioned by stimulus-intensity, by fatigue, or by ennui: and (3) that in the course of adaptation pain is replaced by pressure which is, in the light of the results of our early study,[18] to be interpreted as a residual stimulus-effect.

Since areal, multiple, and punctiform pains behave alike, there is, in our opinion, no need for further duplication with areal and many-pointed stimuli of the experiments on punctiform pain.

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[17] Compare our Table II with their Table I, *op. cit.*, 113.
[18] Stone and Dallenbach, *op. cit.*, 241 f.

THE DIFFERENTIAL REACTION-TIME FOR PLEASANT AND UNPLEASANT WORDS

By M. M. WHITE and MARJORIE POWELL, University of Kentucky

In a recent study, White discovered that pleasant (P) words usually had more associates than unpleasant (U) ones.[1] From that result he concluded that the greater retention of P words in learning experiments, which has been reported by a number of investigators,[2] is due in part to the larger number of associates. Because the number of associates of the two groups of words (the P and U words) overlapped considerably, and because the curves of forgetting of the two groups deviated more and more widely with the lapse of time, White suggested that some other factor, perhaps inhibition, was involved in the forgetting of the U words. The experiments reported in the present study are concerned with the identification of that factor.

EXPERIMENT I

Our first step was to choose pairs of P and U words which had the same number of associates and then to present them to our *S*s until they had been learned to the same degree of mastery. With these two factors (number of associates and degree of learning) controlled, any difference in the retention of the two lists of words (the P and the U) would be evidence of the presence of some other factor or factors. If the U words are forgotten more rapidly than the P words, as the studies mentioned above indicate, either the U experiences are inhibited or the P experiences are facilitated.

In the experiments of White and Ratliff[3] and of White,[4] it was reported that P words were recalled more readily than U words, but that this result held only when both sets of words were learned to the same degree of mastery.

Since forgetting did occur among the P words, it is evident that if facilitation were present it was selective in its action. In the same way, if the difference between

* Accepted for publication August 29, 1934.

[1] M. M. White, Some factors influencing recall of pleasant and unpleasant words, this JOURNAL, 48, 1936, 134-139.

[2] W. D. Tait, The effect of psychophysical attitude of memory, *J. Abnorm. Psychol.*, 8, 1913, 10-37.

R. H. Thomson, An experimental study of memory as influenced by feeling tone, *J. Exper. Psychol.*, 13, 1930, 462-468.

H. Meltzer, Individual differences in forgetting pleasant and unpleasant experiences, *J. Educ. Psychol.*, 21, 1930, 399-409.

M. M. White and M. M. Ratliff, The relation of affective tone to the learning and recall of words, this JOURNAL, 46, 1934, 92-98.

[3] White and Ratliff, *op. cit.*, 93 ff.

[4] White, *op. cit.*

P and U words were due to inhibition, the inhibitory processes must also be selective, that is inhibition must be greater for some *Ss* and for some words than for other *Ss* and for other words. If there is a mechanism of repression, the time necessary to respond to U words will be longer than the time required for P words. Tolman and Johnson found that the free association-time was less for P than for U words,[5] but Birnbaum in a similar study did not obtain corroborative results.[6] It should be stated, however, that apparently the *Ss* used by Tolman and Johnson did not themselves judge the affective tone of the words; it is possible, therefore, that the words, though P and U by some criterion, were not so to every individual in the experiment. Furthermore the alleged P words may have had more associates than the U words. Cason's work has clearly indicated that P experiences are more frequent than U experiences;[7] hence, unless the number of associates is ascertained, it is reasonable to assume that the P words do have more associates. It follows from these facts that although the results of Tolman and Johnson fit in with the hypothesis of a mechanism of repression they do not necessitate such an explanation.

Technique. We have used the Tolman and Johnson technique with several additions. In the first place, we have used words which *Ss* have marked P or U. Secondly, instead of a free-association response being secured, *S* was previously taught a response to make to each P and U word. If the Freudian hypothesis of forgetting is correct, the reaction-time to the U words should be longer than the reaction-time to the P words even immediately after learning the responses to be given to the words. Allegedly, the "censor" begins immediately to repress the unpleasant. Even if the responses made by the *Ss* are neither P nor U, the reaction-time should be longer to the U words, because the responses have been associated with U impressions. One of the assumed laws of repression is that not only the U experience but the system of ideas connected with the U experience is repressed. The minuteness of measurement made possible by the reaction-time technique was indicated by Cattell in 1888.[8] He reported that students showed longer reaction-times in a foreign language than in their native tongue though they wrote and spoke the foreign language fluently. It is probable that by taking the reaction-time we may discover differences between P and U words which are not disclosed by the recall method.

Procedure. The response sheets of 108 *Ss* to whom had been given a list of 10 P and 10 U words with the request to write as many responses to each word as they could think of in half a minute were examined. From these sheets it was possible to select from the women *Ss* 5 pairs of P and U words, each of which had the same number of different associates. Thus "beauty" and "insanity" each had 44 different words given as responses. For the men *Ss*, it was possible to select on this basis only 4 pairs of words. The most frequent response given to each of the 10 words was then noted. For example, to the word "beauty" the response "girl" was given more often than any other. For the reaction-time experiment, each of the

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[5] E. C. Tolman and I. Johnson, A note of association-time and feeling, this JOURNAL, 24, 1918, 187-195.

[6] Cf. J. B. Beebe-Center, *The Psychology of Pleasantness and Unpleasantness*, 1932, 370.

[7] Hulsey Cason, The learning and retention of pleasant and unpleasant activities, *Arch. Psychol.*, 21, 1932, (no. 134), 1-96.

[8] G. T. Ladd and R. S. Woodworth, *Elements of Physiological Psychology*, 1911, 494.

affective words was paired with its most frequently given response. Table I gives the paired words. The P words appear in the first column; the U words in the third column. The first P word listed had the same number of associates as the first U word listed; the second P word had the same number of associates as the second U word, etc. In the second column are listed the response words most frequently given to the P words; in the fourth column are listed the response words most frequently given to the U words. The number in parentheses after each response word indicates how many times that word was given as a response by the 108 *S*s.

The 10 affectively toned words and the responses given to them were arranged in a column and numbered. The first pair was the first combination of a P word with its most frequent response; the second pair was the first combination of an U word and its most frequent response, and so on, alternating between P and U words and their most frequent responses. The pairs were presented auditorially and individually

TABLE I

SHOWING THE LIST OF STIMULUS- AND RESPONSE-WORDS

P words		U words	
Stimulus	Response	Stimulus	Response
1. beauty	girl (26)	1. insanity	crazy (32)
2. smile	laugh (22)	2. fear	afraid (20)
3. lover	boy (14)	3. insult	mad (10)
4. sweet	candy (26)	4. funeral	death (25)
5. father	mother (20)	5. fever	hot (21)

to 16 *S*s. The order in which the pairs were given was determined by shuffling 10 cards before each presentation. The instructions to *S* were as follows. "I am going to read to you a list of paired words. You are to learn the list so that when I give you the first word of any pair, you will be able to say the second word of that pair." After the first presentation, only the first member of each pair was pronounced, and if *S* failed to give the right associate he was corrected. As soon as *S* had given the correct associate, the pair was omitted until all other pairs had been reproduced one time. Then *S* was presented with the entire list once more. This procedure was continued until *S* had given the correct associate for the 10 stimulus-words twice in succession.

After *S* had mastered the series he was seated at the voice-key of the Dunlap chronoscope, the motor of which was connected through a rectifier to an A.C. of 110 volts. He was then instructed as follows: "When I speak the first word of any pair of words you have just learned, you are to say the second word of the pair as quickly as possible. Just before I pronounce the first word of any pair, I shall say, 'Ready.' As soon as I say 'Ready' you are to place your nose here, against the voice key. As soon as you have said the second member of the pair, you may lean back and rest."

The stimulus-words were given twice in random order. The time elapsing between the "ready" signal and the stimulus-word varied from 2-3 sec. *E* recorded not only the reaction-time but the response given by the *S* to each of the 10 stimulus-words.

Results. In Table II, we have placed the mean reaction-time for each of the 16 *S*s for the P and U words. Since the Dunlap motor had 10 poles and

the current used was a 60 cycle one, each complete revolution was 1/5 sec. In other words each 100 units in Table II is equal to 1/5 sec. To get the mean reaction-time for each *S* the two reaction-times obtained for each word were added and averaged, decimals being dropped. All averages thus calculated for P words were added and the mean of them obtained. In the same manner the mean was secured for the U words. In the case of the men there were only four pairs of words, rather than five. When a *S* gave no response or an incorrect one, the reaction-time was not included. To get the mean, the number of reaction-times actually included was divided into

TABLE II

SHOWING FOR EVERY *S* AND FOR THE GROUP OF 16 AS A WHOLE THE MEAN REACTION-TIME FOR P AND U WORDS AND THE DIFFERENCES BETWEEN THESE MEANS

S	Sex	Reaction-time			S	Sex	Reaction-time		
		P	U	Diff.			P	U	Diff.
1	F	695	811	116	9	F	409	618	209
2	F	656	704	48	10	M	471	499	28
3	M	584	911	327	11	M	461	514	53
4	M	494	530	36	12	F	382	507	125
5	M	511	702	191	13	M	453	507	54
6	F	480	471	−9	14	M	462	694	232
7	M	396	440	44	15	M	573	609	36
8	M	726	631	−95	16	M	471	457	−14
						Mean	514	600	86

the total as indicated above. One might argue that if *S* failed to give a response some value should be assigned as reaction-time, because the failure indicated more rapid forgetting. Since, however, the value assigned the forgotten word would be arbitrary and information concerning the number of words forgotten together with their affective tone can be considered independently, it seemed best to omit such cases rather than risk undue distortion of the mean. Sixteen *S*s were unable to give the correct associates 38 times. Of these 38 errors, 23 occurred when the stimulus-word was U and 15 when the stimulus-word was P. For 13 of 16 *S*s the average reaction-time to U words was longer than the average reaction-time to P words. Is this longer recation-time to U than to P words a characteristic of U words or did the association with these particular response words change the affective tone of the words?

To eliminate or at least to minimize the possibility that the responses changed the affective tone of the stimulus-words, two additional experiments, Experiments II and III, were performed.

Experiment II

In Experiment II the stimulus-words[9] used in the foregoing experiment were paired with numbers as responses.

These pairs were presented to a new group of 21 *Ss* by the same method as that used in Experiment I. The reaction-time was taken 5 times for each word instead of twice as in Experiment I. Every time that the series of words was presented, either in learning or in taking the reaction-time, the order of presentation was determined by shuffling cards.

Results. Seventeen of the 21 *Ss* had longer reaction-times for the U than for the P words. Of the 21 *Ss*, 2 made the correct response to every stimulus-word, 5 made more incorrect responses to P words than to U words, and 14 made more incorrect responses to the U words. Of the 270 errors, 154 were to U words. The mean number of incorrect responses to P words per *S* was 5.5, to U words 7.3.

Another way of evaluating the data is proposed by Lange.[10] He suggests that a "coefficient of resistance" be calculated by dividing the median reaction-time into the arithmetical mean reaction-time. A greater variability of reaction-time in the free association test has been found when some of the stimulus-words are loaded. The coefficients of resistance would then be larger for the U than for the P words in this experiment. The mean coefficients for the 21 *Ss* were for P words 1.16 and for U words 1.22.

Of the 21 *Ss*, there were 16 who were able to react correctly to every stimulus-word at least once out of the five opportunities given. Ten of the 16 *Ss* had greater coefficients of resistance for the U words, and 6 for the P words. The mean and median coefficients of resistance for these *Ss* are: P mean, 1.17; P median, 1.16; U mean, 1.21; U median, 1.22. The 5 *Ss* who were unable to react correctly to every stimulus-word failed on one word each; 4 on an U stimulus-word, and 1 on a P stimulus-word. If these 5 *Ss* are treated with the remaining 16 Ss, the coefficient of resistance is greater for U words in 12 *Ss*, and for P words in 9 *Ss*.

These facts indicate that the curves of the reaction-times to P and U words are skewed, and that the skewness is slightly greater for the U words than for the P words. Granting that the coefficient of resistance, which is

[9] While we know that these stimulus-words had the same number of associates for the 108 *Ss* to whom the free association-test was given, we cannot be certain that they all had the same number of associates for the 16 *Ss* selected from that group and used in the reaction-time experiments. It is possible, but improbable since these 16 *Ss* were chosen from the larger group by chance, that the smaller group of *Ss* had more associates for the P than for the U words, though for the group of 108 *Ss* this was not true.

[10] Gardner Murphy and Lois Barclay Murphy, *Experimental Social Psychology*, 1933, 488.

a measure of skewness, is an indication of emotional blocking, the difference between the coefficients, while in favor of the Freudian hypothesis, is so slight as to be easily accounted for on the basis of chance.

EXPERIMENT III

It is rather difficult to get a list of 10 P and 10 U words on which every member of a group of *S*s will give the same number of associates. In consequence of this the words used in Experiment III were varied from *S* to *S*.

A free association-test of approximately 100 words was given individually to 21 *S*s. The directions were as follows: "I am going to read you a list of words. After I have read each word you are to say the first word that comes to mind, and every word after that until I stop you and give you another word. For example, suppose, I read the word 'man,' what is the first word that you think of? After that word, what is next? You understand the words do not have to bear any definite relationship to the word read. Any word you think of will do."

Using the word "man" as an example, the directions were supplemented until it was apparent that *S* understood that any word would do, that he was to continue the association process until he was stopped and given a new word. The time given for each word was half a minute. *E* recorded the number of words or phrases spoken, not the particular words or phrases. As soon as the association-test was completed *E* read each stimulus-word aloud, and asked *S* to indicate whether the word was P, U, or I.

In so far as possible 5 pairs of P and U words, each of which had the same number of associates were selected. In 5 instances, it was impossible to get 5 pairs of P and U words. In 3 instances, 4 pairs were obtained; and in 2 instances only 3 pairs. These words were then paired with numbers, an attempt being made to equate the difficulty of the numbers in the two lists. Hartman has arranged the numbers from 1 through 99 in order of ease of recall.[11] From this order, numbers were so chosen that the difficulty of the 5 numbers paired with the P words was approximately the same difficulty as the 5 numbers paired with the U words. Furthermore, the numbers paired with P words with one *S* were paired with U words for the next *S*. For example, the number "61" appeared with a P word for *S* 9, and with a U word for S 10.

Immediately after the construction of these pairs, the words and their numbers were presented to *S* for learning in the same manner as in the preceding experiments. The reaction-time was taken 3 times for each word by the same method as that used in the other experiments.

Results. Table III gives the mean reaction-time of each *S* to the P words and to the U words. It will be noticed that 19 out of 21 *S*s had longer reaction-times to the U than to the P words. Of the 109 errors, 56 occurred in response to U words, and 52 to P words. Of the 21 *S*s, 14 made more incorrect responses to U words than to P words, 1 made an equal number of

[11] George W. Hartman, Determination of easy and difficult number combinations by immediate auditory memory, *Proc. 9th Inter. Cong. Psychol.*, 1929, 210.

incorrect responses, and 6 made more incorrect responses to P words than to U words.

In 68 of the 93 pairs of words that were reacted to correctly, the reaction-time was longer to the U words, and in 25 it was longer to the P words. Of the 68 numbers forgotten, 37 were in response to U words, and 31 to P words.

Summary

In a previously reported experiment in which were used two different lists of P and U words and two different groups of *Ss*, it was found that more U than P words were forgotten and that the discrepancy between the two increased with the elapse of time. Another study made in the attempt to explain the above difference indicated that P words have more associates than U words, but there was considerable overlapping. The present experiment had for its purpose a further isolation of the factors determining the differential recall of P and U experiences.

In this experiment 5 U words were matched with 5 P words having the same number of associates. The responses most frequently given by 108 *Ss* in the experiment previously reported were paired with these words. After 16 *Ss* had learned the 10 pairs the reaction-time on each pair was taken. The same procedure was repeated twice with different groups of *Ss*: once using the same stimulus-words paired with numbers; and again using different stimulus-words equated for each S in terms of the number of associates. It was found that (1) the average reaction-time was smaller for the P words than for the U words whether words or numbers were used as responses and whether the number of associates was equated in terms

TABLE III

Showing for Every *S* and for the Group of 21 as a Whole the Mean Reaction-Time for P and U Words and Their Difference When the Response was a Number

S	Sex	Reaction-time			S	Sex	Reaction-time		
		P	U	Diff.			P	U	Diff.
1	M	604	2415	1811	12	F	611	746	135
2	M	443	513	70	13	M	632	843	211
3	M	516	532	16	14	M	755	1236	481
4	M	517	686	169	15	M	676	506	−170
5	M	525	461	−64	16	M	406	780	374
6	M	823	1382	559	17	M	501	644	143
7	F	669	785	116	18	M	669	919	250
8	M	480	629	149	19	M	460	675	215
9	M	447	582	135	20	F	569	779	210
10	M	466	540	74	21	M	611	759	148
11	M	560	911	351					
						Mean	568	824	256

of the group as a whole or the individual *S;* (2) of 58 *S*s, 49 had longer average reaction-time to the U than to the P words; and (3) more incorrect responses were made to U than to P words.

The results of this experiment, like those of the experiments reported in the two other papers of this series, are consistent with the hypothesis that there is an inhibition of the unpleasant. Although the difference between U and P has never been great nor occurred for all *S*s, it has been thoroughly consistent and where we have used a large number of *S*s it has been statistically reliable.

SOME FACTORS INFLUENCING RECALL OF PLEASANT AND UNPLEASANT WORDS

By M. M. WHITE, University of Kentucky

Many writers have called attention to the fact that the relation of affective tone to recall is still undetermined because of conflicting results of experiments. Bills states, for example, that "the results of studies of affective tone and retention are extremely contradictory."[1] In an experiment reported by White and Ratliff,[2] it was suggested that four factors are predominantly responsible for the conflicting results: (1) incorrect assumption of the affective tone of the material used; (2) variations in familiarity with the material; (3) crude measuring instruments; and (4) different instructions. White and Ratliff found that, with instructions to master, there was practically no difference in the ease with which P and U experiences were learned, but that the U experiences were forgotten more readily and as the time-interval between recall and learning increased, the curve of forgetting for the U experiences became more widely separated from that of the P experiences. The experiments to be reported here are concerned with the question: Why did the curves become more and more widely separated?

Some possible explanations are: (1) It might be due to the fact that the same *Ss* were not tested after the various intervals of time. One group of *Ss* was tested at the end of 7 days, another at the end of 14 days, another at the end of 21 days, and a fourth group at the end of 28 days. (2) The results might be due to the fact that the *Ss* had formed more associations with the P than with the U material by virtue of everyday experience. Thus, the word "flower" is likely to have more associates than the word "insanity." (3) It could be due to the fact that there was active forgetting. (4) It might be due to some undetermined peculiarity of our list of words. We have attempted to isolate these factors in a series of experiments. The first experiment to be reported is concerned with the possibility that the curves were the result of having different groups of *Ss* recall after the various intervals of time.

EXPERIMENT I

Each of 30 words was typed on a 3 by 5 index card. Ten of the words were P, ten U, and ten I. The list of words was the same as that used in the experiment previously reported by White and Ratliff. Since we have been unable to obtain a list of 10 words which a large number of *Ss* will consistently mark indifferent, we have disregarded all results pertaining to them.

Eight *Ss*, 5 men and 3 women, were asked to take part in an experiment on learning.[3] The 30 words arranged in a random order were visually presented to the group. After each presentation the *Ss* wrote as many of the words as they could

* Accepted for publication August 2, 1934.

[1] A. G. Bills, *General Experimental Psychology*, 1934. 337.

[2] M. M. White and M. M. Ratliff, The relation of affective tone to the learning and recall of words, this JOURNAL, 46, 1934, 92-98.

[3] The writer wishes to thank Dr. A. B. Crawford for his assistance in this experiment.

recall. When any *S* succeeded in recalling all 30 he was dismissed with the instructions not to think of them again. The *S*s were not told that they would be tested again but probably the instructions "not to think of the words again" caused them to suspect a retest.

The next day the *S*s were given a sheet of paper on which they were requested to write as many of the words as they could recall. Reproduction was required again 8, 15, 29, and 43 days after learning. The intervals between the various recall periods were then 1, 7, 7, 14, and 14 days. The *S*s had no way of determining how successful they were in any of the recall periods. There were no more presentations after the original learning.

TABLE I

COMPARISON OF NUMBER OF WORDS RECALLED BY 8 *S*s ACCORDING TO THE INTERVAL OF DELAY

	Days after learning				
	1	8	15	29	43
P Words recalled	71	63	63	62	62
U Words recalled	68	53	54	52	46
Diff.	3	10	9	10	16
% P	51.08	54.31	53.84	54.38	57.40

The results are given in Tables I and II. Table I contains the total number of P and U words recalled on the various days. It gives also the number of P words recalled in terms of the percentage of the total number of recalled words. Thus, we see that after an elapse of one day 71 P words and 68 U words were recalled by the *S*s; of the words recalled after one day 51% were P. After 43 days, 62 P and 46 U words were recalled; the 62 P words are 57% of the total reproduced.

TABLE II

COMPARISON OF NUMBER OF WORDS RECALLED AFTER 43 DAYS IN TERMS OF THE PERCENTAGE OF WORDS RECALLED AFTER ONE DAY

Words recalled	Subject								Mean
	1	2	3	4	5	6	7	8	
P	80	80	110	90	80	100	80	80	87.5
U	40	60	70	90	90	100	60	70	72.5
Diff.	40	20	40	00	−10	00	20	10	15.0

Table II contains a comparison of the number of words recalled after 43 days in terms of the percentage of words recalled after one day. One *S* recalled a greater percentage of U than P words; 5 recalled a greater percentage of P words; 2 recalled the same number of each. The mean for P words was 15.0% greater than that for U words. Since forgetting is usually more rapid immediately after learning than at any other time, the difference between the two means is the more significant because it was based on the forgetting which occurred after an interval of one day.

One of the criticisms which have been advanced against the experiments on the recall of P and U experiences is that the situations have been too artificial; that is,

not sufficiently like everyday experience. The significance of this criticism may be determined to some extent by comparing the results of two experiments one of which is more common to usual experience than is the other.

If we permitted a group of *Ss* to take the list of words home, memorize the words there, and report to the laboratory next day, we would have an experimental situation which resembles everyday experience more than does the preceding experimental situation. To be sure, such procedure means that the *Ss* will probably overlearn some of the words and underlearn some of the others, and that the technique used in mastering the words will vary from *S* to *S*. If, however, with these variations we still obtain the same results, the validity of the conclusions reported above will be strengthened.

TABLE III

COMPARISON OF NUMBER OF WORDS RECALLED BY 11 *Ss* ACCORDING TO THE INTERVAL OF DELAY

N	Days after learning				
	1	8	15	29	43
P Words recalled	104	87	88	86	82
U Words recalled	103	85	79	71	70
Diff.	1	2	9	15	12
% P	50.24	50.58	52.69	54.77	53.94

TABLE IV

COMPARISON OF NUMBER OF WORDS RECALLED AFTER 43 DAYS IN TERMS OF THE PERCENTAGE OF WORDS RECALLED AFTER ONE DAY

Words recalled	Subject											Mean
	1	2	3	4	5	6	7	8	9	10	11	
P	100	100	67	100	133	90	100	86	80	100	100	96.0
U	90	88	50	110	90	100	88	67	80	80	100	85.7
Diff.	10	12	17	−10	43	−10	12	19	00	20	00	10.3

Eleven *Ss* were given the list of 30 words with the instructions to take them home, memorize them and report to the laboratory at the same hour the next day. The procedure otherwise was the same with these 11 *Ss* as with the preceding 8. The results are given in Tables III and IV. We see that the percentage of the words recalled that were P increased from 50 to 54%. The superiority of P words in recall increased gradually but there was a slight decrease after 43 days as compared with 28 days. Table IV shows that 2 out of the 11 *Ss* recalled a greater percentage of U than P words at the end of 43 days; that 7 recalled a greater percentage of P words; and that 2 recalled the same number of each. The difference between the means is 10.3%.

The results from these two groups of *Ss* indicate that (1) the results previously obtained by White and Ratliff cannot be explained by the fact that they used different *Ss* for the various intervals of delay; (2) U words are more likely to be forgotten than P words. The conditions determining the difference are still to be discovered.

The difference may be due to the fact that P experiences have more associations than U ones. For example, if experience A is associated with 20 other experiences and experience B with only 5, we should expect experience A to be more readily recalled, other things being equal. Is the number of associations a factor determining our results? The following experiment was performed to test this possibility.

Experiment II

The 20 P and U words were arranged in random order. The list was auditorially presented to 123 *Ss*, 67 men and 56 women, in groups ranging from 15 to 20. The instructions to the groups were as follows:

I am going to read you a word. Write it at the extreme left of your paper. After it, write as many words as the word makes you think of. The words you write in response to the stimulus-word do not have to be logically related to the stimulus. Any word that comes to mind will do. You will be given a half-minute for each word.

TABLE V

Mean Number of Responses for Every U and P Word

U words	Mean no. responses		P words	Mean no. responses	
	men	women		men	women
funeral	6.8	7.5	home	6.9	7.0
dead	6.5	6.7	flower	6.9	7.0
fever	6.4	6.5	eat	6.5	7.0
poison	5.8	5.9	smile	5.5	6.0
terror	5.6	5.8	beauty	5.5	5.7
fear	5.0	5.8	sweet	5.4	6.0*
insanity	5.0	5.4	laugh	5.4	5.1*
insult	4.6	4.9	girl	5.2	6.5*
vomit	4.6	4.5	father	5.2	6.0*
disgrace	4.5	4.8*	lover	5.2	5.3
Total	54.8	57.8	Total	57.7	61.6

* The words are arranged in descending order of number of responses for the men. Among the women the displacement of a word on this basis is indicated by the asterisk.

Table V shows the mean number of words written in response to each of the stimulus-words. Since girls have been found to be superior in language to boys,[4] we have calculated the means of the two sexes separately. It was necessary to discard the results of some *Ss* for some of the words because they failed to write the correct stimulus word. A frequent error was writing the word "love" for "lover." In Table V, consequently, the number of *Ss* on which the average for each word is based varies from 108 to 123. Five U words have fewer associates than any P word for the boys, and 3 U words have fewer associates than any P word for the girls. Two P words have more associates than any U word among the men; among the women the U word, "funeral" has the greatest number of associates; excepting "funeral," 3 P words have more associates than any U word. In spite of this great overlapping, the average number of associates for P words is larger than for the U words for both men and women.

[4] G. M. Whipple, *Manual of Mental and Physical Tests*, 2, 1916, 213.

Table VI gives the mean for P and U words for the 108 Ss who wrote responses for the entire list of words. The mean of responses to P words is 5.21 words higher than the mean of the responses to the U words and the chances are 99 out of 100 that this difference is a true one. Seventy-three of the 108 Ss wrote more responses to the P than to the U words, 30 wrote more responses to the U words, and 5 wrote the same number of responses to the P as to the U words.

TABLE VI

COMPARISON IN TERMS OF MEANS OF U AND P RESPONSES FOR 108 Ss

U mean	S.D.	S.E.	P mean	S.D.	S.E.	$S.E._{diff.}$	Ob. Diff.
57.90	8.80	.847	63.11	9.01	.867	1.21	5.21

The question then arises as to whether these results are peculiar to the particular word list or whether any other list made in the same manner is likely to give the same results. To answer this query another list was made. Each of 12 people was asked to hand to the experimenter a list of 10 words that were pleasant and 10 words that were unpleasant. From this total of 240 words a final list of 144 was secured after the duplicates were discarded. These 144 words were placed in chance order in a column on the left margin of a sheet. After each word were five boxes which were headed "very pleasant," "pleasant," "indifferent," "unpleasant," and "very unpleasant," respectively. The words were then presented to 100 Ss, 50 women and 50 men, with the following instructions:

In the column below are some words. After each word are 5 possible boxes for you to check each word. Check each word in one of the 5 boxes depending upon whether that word appeals to you as being very pleasant, pleasant, indifferent, unpleasant, or very unpleasant.

From the list of 144 words it was possible to select 9 words which every student stated were U and 10 words which every student stated were P. In order to have an equal number of P and U words, one P word, "honor," was discarded.

These 18 words were presented to 103 Ss, 60 men and 43 women in the same way that the list of 20 words had been previously presented. The results appear in Table VII.

TABLE VII

MEAN NUMBER OF RESPONSES FOR EVERY U AND P WORD

U words	Mean no. responses		P words	Mean no. responses	
	men	women		men	women
spit	6.8	7.0	dance	8.3	7.8*
sin	6.7	5.2*	sunshine	7.8	7.8*
pox	6.5	5.8*	marry	7.5	9.2
drunk	6.1	7.7*	friend	7.2	8.6
afraid	6.1	6.5	theatre	7.0	8.4
cheat	6.0	6.1	succeed	7.0	7.3
indecent	6.0	5.5	kiss	6.6	5.9
nausea	5.5	5.9*	love	6.4	7.2*
illegitimate	5.0	4.5	mother	5.7	8.8*
Total	54.7	54.2	Total	63.5	71.0

* The words are arranged in descending number of responses for the men. Among the women the displacement of a word on this basis is indicated by the asterisk.

Since some of the words were again misunderstood by some of the *Ss* the number of *Ss* on whom the mean for each word is based varies from 75 to 103. Table VIII gives the means of the responses to the P and U words based on 75 *Ss* who understood correctly the entire list of words. The difference between the two means is 11.43 words in favor of the P words, a corroboration of the results reported above. Of the 75 *Ss*, 65 gave more responses to the P words, 8 gave more to the U words, and 2 gave the same number of responses to each.

TABLE VIII

COMPARISON IN TERMS OF MEANS OF U AND P RESPONSES FOR 75 *Ss*

U mean	S.D.	S.E.	P mean	S.D.	S.E.	$S.E._{diff.}$	Ob. Diff.
55.54	20.09	2.31	66.97	25.65	2.96	3.75	11.43

We may conclude that one of the factors which must be considered in accounting for the greater recall of the P words is the greater number of associates for the P words. Using two different lists of words, presenting them to two different groups of *Ss*, more responses were given to the P than to the U words; and of the 183 *Ss* 138 wrote more responses to P than to U words. If the superior number of associates for P words is one factor in the greater recall of P words, is it the only factor? Probably not for there is great overlapping of the P and U words in the number of associates given to each. The next step then, is to choose P and U words which have the same number of associates and determine if the P words still hold their superiority in reproduction. A paper which will appear at an early date reports the results of such a study.

REACTION-TIME TO RETINAL STIMULATION UNDER LIGHT AND DARK ADAPTATION

By VERNON W. LEMMON and SAMUEL M. GEISINGER, Washington University

It has been demonstrated by Poffenberger[1] and others that the reaction-time to peripheral stimulation of the retina is greater than to foveal stimulation. Most if not all such measurements, however, have been made under conditions of light adaptation. It is also generally accepted that when the retina is dark adapted, the periphery is more sensitive to faint light than the fovea. Astronomers, for example, know that a faint star can be seen more readily in indirect vision. This suggests the possibility that the periphery might give a shorter reaction-time than the fovea under conditions of dark adaptation. Poffenberger makes this suggestion,[2] but it does not seem to have been followed up by other investigators. The present study is an attempt to investigate this question.

Procedure. The *Ss* were 14 college students. The reaction consisted in raising the hand from a telegraph key as soon as a light-stimulus appeared. The reaction-times were measured by a Dunlap chronoscope which was situated in a separate room from the *S*, in order to avoid auditory and other secondary cues. Measurements were made under both light and dark adaptation. The former served as a control and as a check on earlier results. Forty-eight reactions were made by each *S* under each condition of adaptation, giving a total of 624 reactions under light and 672 under dark adaptation. (The difference between these figures is due to the fact that one *S* reacted under dark adaptation only.) An ABBA order was used to equalize the practice effect between fovea and periphery. As an additional precaution, about half of the *Ss* reacted first under light adaptation, and the other half first under dark adaptation.

The experimental room was painted black and had artificial illumination only. For the light-adaptation measurements, general illumination was provided by two large overhead lights. In addition, two large white sheets were suspended about 2 M. in front of *S*, filling practically the entire visual field. Two 200-watt lamps backed by white reflectors were placed behind *S*; these shone on the sheets and provided a brilliantly illuminated field.

S was seated in a chair with his head in a rest. The stimulus was a mazda flashlight bulb in a small box with a white front in which was cut a circular aperture 1 cm. in diam., backed by a sheet of opal glass. For foveal stimulation, this aperture was placed directly in front of *S* at a distance of 58 cm. from his eyes. According to Poffenberger's calculations,[3] this would result in the image of the aperture falling entirely within the fovea. Both eyes were used together in all measurements. The intensity of the stimulus was not measured, but it was sufficient to be plainly visible against the background.

For peripheral stimulation *S* kept his eyes directed straight ahead, fixating on a

* Accepted for publication August 25, 1935.

[1] A. T. Poffenberger, Reaction-time to retinal stimulation, *Arch. Psychol.*, 3, 1912 (no. 23), 1-73.

[2] *Op. cit.*, 55.

[3] *Op. cit.*, 35 f.

pin-head in a white card, 58 cm. from his eyes. The stimulus-box was moved to the left until the line from it to the eyes made an angle of 45° with the line of sight. The aperture was still 58 cm. from the eyes.

A few practice reactions were given to accustom *S* to the situation. A warning signal was given by means of a buzzer; approximately 2 sec. later the stimulus-lamp was lit and the chronoscope started simultaneously. The lamp remained lit until the reaction had been made. There was of course a slight lag in the lighting of the lamp, but, since it was substantially constant for all measurements, it could be neglected.

For the dark-adaptation measurements the room was, of course, absolutely dark.

TABLE I

AVERAGE REACTION-TIMES IN SIGMA

S	Light adaptation		Dark adaptation	
	fovea	periphery (45°)	fovea	periphery (45°)
1	195.22	213.59	350.70	354.37
2	195.39	225.78	362.39	352.37
3	174.51	198.73	244.15	284.40
4	247.66	267.36	436.20	459.25
5	241.48	231.29	446.72	430.52
6	201.73	239.64	386.60	350.70
7	189.71	198.22	380.42	384.43
8	294.42	303.60	515.19	454.24
9	263.02	409.98	506.84	478.45
10	190.38	222.94	328.99	349.86
11	207.91	239.64	402.47	373.24
12	184.53	252.17	383.26	404.14
13	189.54	227.95	461.75	418.33
14	—	—	357.04	364.56
Av.	213.64	248.64	399.01	388.69
Diff.	−35.00		10.32	
S.D.$_{diff.}$	5.16		6.16	
Ratio	6.78		1.67	

S was required to become adapted for 30 min. before any measurements were taken. The stimulus-box had a black instead of a white front and the intensity of the light was reduced by inserting behind the aperture an absorbing screen of several thicknesses of ground glass and tissue paper. The intensity of the resulting stimulus was not measured, but it was so faint as to be just barely visible on the fovea after the adaptation-period. It could not be seen at all at the beginning of this period. This intensity could scarcely have been sufficient to destroy the dark adaptation to any appreciable extent. A fixation-point was provided by a pin-head, faintly illuminated by a fine pencil of light coming from a box located off to one side and so placed that no stray light was visible. Otherwise the procedure was the same as for light adaptation.

Results. The average reaction-times for each *S* under each condition and final averages for all the *S*s are given in Table I. Considering first the results for light adaptation, it is seen that every *S* but one gave a slower reaction to peripheral than to foveal stimulation. The difference between the final averages is 35 sigma, which

is in the same direction as, though slightly larger than, the difference found by Poffenberger.[4] The ratio of this difference to its standard deviation is 6.78, indicating a high degree of reliability. (All standard deviations were computed from distributions of the individual measurements.)

In the case of dark adaptation, half of the *S*s showed quicker reactions to peripheral stimulation, and half showed the reverse relationship. The differences, however, indicating faster peripheral reactions were in general the larger; the final averages show peripheral reactions to be 10.32 sigma quicker than foveal. This difference is 1.67 times its standard deviation; while falling short of complete statistical reliability, it indicates that the chances are about 95 in 100 that the difference is a true one.

Summary and conclusions. Measurements were made of reaction-times to peripheral and foveal retinal stimulation under conditions of both light and dark adaptation. The final averages showed that under light-adaptation reactions to foveal stimulation were reliably faster by 35 sigma, thus confirming previous work. Under dark adaptation, however, reactions to peripheral stimulation were faster by 10.32 sigma, and the reliability was such as to indicate at least a strong probability that this difference is a true one.

[4] *Op. cit.*, 48.

APPARATUS

AN ELECTRONIC VOICE-REACTION TIMER

By HARRY R. DESILVA and LARRY JACOBSON, Massachusetts State College

This note describes a later model of an electronic voice-reaction timer described in a previous issue of this JOURNAL.[1] It represents improvements in the following respects: (1) It is more convenient and economical to operate, utilizing A.C. power entirely. (2) The microphones are more easily adjusted over a wider range of sound energy. (3) It is more compact. (4) By employing an improved synchronous motor-

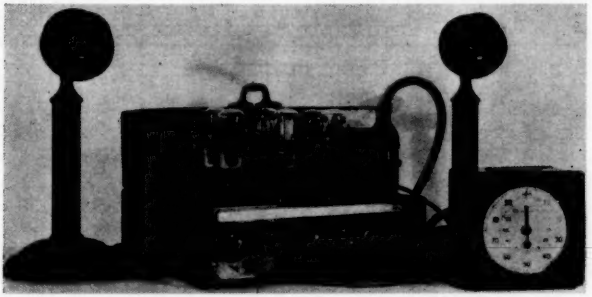

FIG. 1. THE VOICE-REACTION TIMER

clock and electronic relays the timer is made to operate more accurately and without noise.[2]

The unit is illustrated in Fig. 1. A diagram is given in Fig. 2 followed by a table of parts. When *E* speaks into his microphone, plugged into J_1, the voltage generated is amplified and triggers a gaseous triode in whose anode circuit is a clutch coil. The clutch, when energized, couples a synchronous clock-motor to its two hands, reading in hundredths of a second, and seconds.[3] The *S* replying into a microphone, plugged into J_2, triggers a second gaseous triode. Its function is to stop the clock by extinguishing the first gas tube, which is then locked out by the relay. The clock cannot again be started until *E* momentarily depresses the key switch which resets the timer.

In constructing the timer, the audio transformers should be mounted at right

[1] H. R. DeSilva, A timer for vocal reactions with thyratron tubes and microphones, this JOURNAL, 47, 1935, 315-317.

[2] These improvements were made possible by the financial assistance of the Massachusetts Emergency Relief Administration.

[3] We used the Standard Electric Time Co. Clock, Model SC-1.

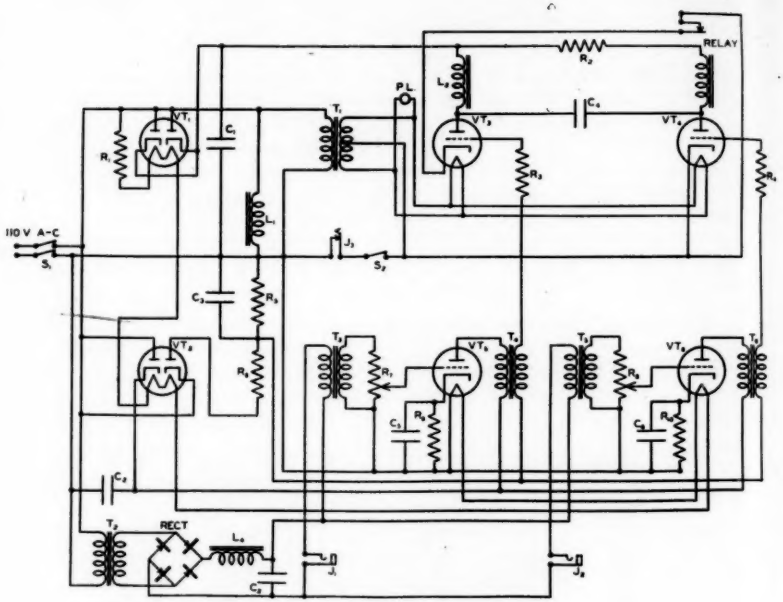

FIG. 2. WIRING DIAGRAM OF TIMER

VT_1, VT_2 = type 25Z5 vacuum tubes
VT_3, VT_4 = type 885 vacuum tubes
VT_5, VT_6 = type 76 vacuum tubes
C_1, C_2 = 32-mfd., 200-volt, electrolytic condensers
C_3 = 16-mfd., 50-volt, electrolytic condenser
C_4 = 0.5-mfd., 400-volt paper condenser
C_5, C_6 = 25mfd., 25-volt, electrolytic condensers
C_7 = 1000-mfd., 12-volt, electrolytic condenser
R_1 = 177-ohm, 75-watt resistor (200-ohm resistor, adjust for 0.3 amps)
R_2 = 1000-ohm, 25-watt resistor
R_3, R_4 = 500,000-ohm, 1-watt resistors
R_5, R_6 = 3100 ohms and 4500 ohms, respectively, of a 7000-ohm, 25-watt resistor
R_7, R_8 = 150,000-ohm volume controls
R_9, R_{10} = 1500-ohm resistor, 1 watt
L_1 = Field of clock motor
L_2 = Clutch-coil of clock, 85A coil
RELAY = Dunco midget relay, 60A coil, S.P.S.T. break
L_4 = 1.5 Henry choke, 200 ma., 35 ohms (approx.)
T_1 = Filament transformer, line voltage to 2.5 volt at 0.3 ampere
T_2 = Transformer, line voltage to 7.5 volt at 0.25 ampere
T_3, T_5 = Single-button microphone to grid transformer
T_4, T_6 = 2:1 interstage audio transformer
S_1 = D.P.S.T. toggle switch
S_2 = S.P.S.T. toggle switch
J_1, J_2 = Open circuit jack
J_3 = Closed circuit lever type key switch
RECT. = Copper-oxide rectifier, type F-16, B-L Electric Mfg. Co.
P.L. = 2.5 volt, pilot light

angles to the power transformers. R_1 should not be mounted close to the electrolytic condensers. Care should be taken that the copper-oxide rectifier is properly connected. The output voltage of the filament transformer should be closely set to 2.5 volts at the prevailing line voltage. If a clock with an electrical reset is used, 0.001 mfd. condensers should be connected between grid and cathode of VT_3 and VT_4 to prevent transient impulses from triggering these tubes.

In operating the timer, switch S_2 is closed 1 min. after line switch S_1, to allow the cathodes VT_3 and VT_4 to reach operating temperature. The volume controls should be set for the particular conditions of the test, *i.e.* the voice of *S* and of *E*, and the noise level of the room.

The sensitivity of the timer is such that the clock can be controlled by the low sound energy contained in transitional sounds at the beginning of words. The noise made by the clock and its clutch mechanism has not been measured. It is, however, imperceptible unless the timer is operated in a quiet room. The standard clutch mechanism has been quieted by fixing a leather disk to the pole face of the clutch relay.

Without the observance of special precautions in the interest of accuracy, voice-reactions can be measured with an error not exceeding 0.005 sec. for relatively small intervals upward from 0.005 sec., when the line frequency is maintained reasonably close to 60 cycles. The smallest divisions of the clock dial correspond to hundredths of a second. Fifths of a division can be estimated.

The timer described has been successfully operated by students in measuring vocal and other reactions in our laboratory and many thousands of measurements have been made with it. The cost of the timer, including clock and microphones, is moderate.

TWO NEW A.C. CHRONOSCOPES

By KARL M. DALLENBACH, Cornell University

The purpose of this note is to call attention to two 'Standard Electric' clocks which have recently appeared.[1] We have had these clocks in the Cornell Laboratory since last spring and we have found them rugged and well-suited for demonstrational and experimental work.

Both models are operated by self-starting telechron motors[2] which run continuously during the time the apparatus is set up ready for use. The hands of the chronoscopes, a large second-hand and a small minute-hand, are driven through a horizontally mounted friction-clutch that is actuated magnetically by a 6-volt D.C. current. When

[1] The clocks are made by Standard Electric Time Co., Springfield, Massachusetts. The price is relatively low, largely due to the fact that the clock-mechanism is a standard part of a cycle-counter, made by the same firm, that is widely used in the electrical industry, and is consequently manufactured on a production-basis.

[2] Cf. W. L. Valentine, A small self-starting synchronous motor, this JOURNAL, 40, 1928, 495 f. L. W. Max has recently described a chronoscope in which a telechron motor was similarly used (A self-starting chronoscope, *ibid.*, 47, 1935, 154 f.).

TABLE I

SHOWING 40 MEASUREMENTS FROM A SERIES OF 100 THAT WERE MADE WITH THE 'STANDARD TIME' STOP-CLOCKS AND 10 MEASUREMENTS WITH THE CORNELL CHRONOSCOPE AT SETTING A OF THE CONTROL HAMMER

(Arranged in order of the first three groups and the last group of 10 measurements)

No. within series	Model SWC-1 (1/100 sec.)				Model SC-1 (1/300 sec.)				Cornell Chron. (1/1000 sec.)
	1st 10	2nd 10	3rd 10	10th 10	1st 10	2nd 10	3rd 10	10th 10	1st 10
1	79	69	68	68	206	201	199	198	682
2	78	69	69	68	207	199	199	200	680
3	75	70	70	69	204	199	199	198	678
4	68	70	69	70	203	197	198	199	680
5	68	70	69	69	200	198	198	198	676
6	71	69	69	69	200	198	198	199	678
7	71	70	69	70	200	198	197	198	671
8	70	70	69	69	200	197	199	199	671
9	71	69	70	69	200	196	200	200	672
10	69	70	69	69	200	196	199	198	676
Av.	72.00	69.60	69.10	69.00	202.00	197.90	198.60	198.70	676.40
m.v.	3.20	0.48	0.36	0.40	2.40	1.12	1.08	0.70	3.20
% var.	4.44	0.68	0.52	0.57	1.19	0.57	0.54	0.35	0.48

TABLE II

SHOWING 40 MEASUREMENTS FROM A SERIES OF 100 THAT WERE MADE WITH EACH OF THE 'STANDARD TIME' STOP-CLOCKS AND 10 MEASUREMENTS WITH THE CORNELL CHRONOSCOPE AT SETTING B OF THE CONTROL HAMMER

(Arranged in order of the first three groups and the last group of 10 measurements)

No. within series	Model SWC-1 (1/100 sec.)				Model SC-1 (1/300 sec.)				Cornell Chron. (1/1000 sec.)
	1st 10	2nd 10	3rd 10	10th 10	1st 10	2nd 10	3rd 10	4th 10	1st 10
1	13	13	13	13	36	35	35	35	133
2	13	13	13	13	34	36	35	36	131
3	14	13	13	13	36	35	35	35	130
4	13	13	14	13	35	35	36	35	129
5	13	13	13	13	35	35	35	35	131
6	14	13	14	14	36	36	36	35	130
7	13	13	13	14	37	36	35	34	129
8	14	13	13	14	36	35	34	35	128
9	14	14	13	13	37	36	35	35	130
10	13	14	14	13	35	35	36	35	129
Av.	13.40	13.20	13.30	13.30	35.70	35.40	35.20	35.00	130.00
m.v.	0.48	0.32	0.42	0.42	0.76	0.48	0.48	0.20	1.0
% var.	3.61	2.42	3.15	3.15	2.13	1.35	1.36	0.57	0.77

TABLE III

THE MEASUREMENTS REDUCED FOR COMPARATIVE PURPOSES TO 1/1000 SEC.

Setting	Chronoscope	Measure	1st 10	2nd 10	3rd 10	10th 10	last 90
A	SWC-1	Av.	720.0	696.0	691.0	690.0	690.88
		m.v.	32.0	4.8	3.6	4.0	3.82
		% var.	4.44	0.58	0.52	0.57	0.55
A	SC-1	Av.	673.3	659.6	662.0	662.3	661.46
		m.v.	8.0	3.7	3.6	2.3	2.84
		% var.	1.19	0.57	0.54	0.35	0.43
A	Cornell	Av.	676.4				
		m.v.	3.2				
		% var.	0.48				
B	SWC-1	Av.	133.0	132.0	133.0	133.0	132.55
		m.v.	4.8	3.2	4.2	4.2	3.97
		% var.	3.61	2.42	3.15	3.15	2.99
B	SC-1	Av.	119.0	118.0	117.3	116.66	116.66
		m.v.	2.53	1.60	1.60	0.67	1.74
		% var.	2.13	1.35	1.36	0.57	1.48
B	Cornell	Av.	130.0				
		m.v.	1.0				
		% var.	0.77				

the circuit is closed, the clutch engages and the hands start instantly and continue to revolve until the circuit is broken. The clutch is then disengaged and held fast by means of a retraction-spring.

The second-hand makes one revolution per second, and the minute-hand one per minute. In the large model (Model SWC-1) the dial is 10 in. in diam., with 100 divisions for the second-hand and 60 for the minute-hand. In the small model (Model SC-1) the dial is $3\frac{3}{16}$ in. in diam., with 300 divisions for the second-hand and 60 for the minute-hand. When the clocks are operated with a 60-cycle A.C. current they give values of 1/100 and 1/300 sec. respectively for intervals up to 60 sec.

Both models have simple reset devices for returning the hands to the zero-point after every measurement. The small model is reset mechanically by giving a quarter-turn to a small lever-arm projecting from the front of the case. This returns the hands exactly to the zero-point. The large model is reset electrically. This is less accurate than the mechanical reset and care must be taken to see that the hands are returned to zero. The electrical reset is operated by the A.C. current used to run the clock and it has the advantage that it may be operated at a distance from the clock by means of an extension-cord and attached push-button. Because of this reset arrangement, as well as its size, the large clock may be placed high on the wall of a class or lecture room in clear view and used to advantage in demonstrations.

We have made extensive tests of these clocks and we have found them, on the whole, highly satisfactory. Variability was tested with Wundt's large control hammer,[3] and accuracy was checked with a Cornell A.C. Chronoscope.[4] The control hammer was set for various intervals of time, but in the accompanying tables we give the results of only two settings: Setting A, a long interval as reaction-time measurements go; and Setting B, a short interval. We used Setting A first and then Setting B. With each setting we took 100 measurements from the large model (SWC-1), 100 from the small model (SC-1), and 10 from the Cornell chronoscope.

For statistical treatment, the 100 measurements from each model were divided into 10 consecutive groups of 10 measurements each. The average, m.v., and percentage of variation were computed separately for every group and for the last 90 combined. In the accompanying tables, we give these values together with the crude data for 4 of the 10 groups—the first three and the last one. In the intervening groups (4th to 9th inclusive) the results are so much like those from the 3rd and 10th that we omit them. Table I refers to Setting A, and Table II to Setting B. Table III summarizes Tables I and II and gives the results of the last 90 measurements. The units of time, however, are here reduced, for ease of comparison, to the common denominator of 1/1000 sec.

An inspection of the tables brings out the following facts:

(1) In the first group of 10 measurements with both of the Standard Time models and both settings of the control hammer, the average, m.v., and percentage of variability are significally larger than in the subsequent 9 groups.

(2) In groups 2 to 9, averages, amounts and percentages of variability do not vary significally.

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[3] For a description of this apparatus and an explanation of the method of using it see W. Wundt, *Grundzüge der physiologischen Psychologie*, 3, 1911, 374 ff.

[4] K. M. Dallenbach, The Cornell A. C. chronoscope, this JOURNAL, 47, 1935, 319-322.

(3) The length of the individual measurements decreases markedly during the first 10 trials with Setting A: from 0.79 to 0.68 sec. with the large model; from 207/300 to 200/300 sec. with the small model; and from 0.682 to 0.671 sec. with the Cornell chronoscope. The decreases are 110σ, 23σ, and 11σ respectively. With Setting B, the divergence is similar but not so great. It should be recalled that 100 measurements were made with each Standard Time clock at Setting A before Setting B was used.

Results similar to those reported here were obtained at every test-period. The irregularity of the first group of ten measurements suggests a 'warming-up' effect. The locus of this variable error must be placed in the clutch, because all other parts of the clock-mechanism were thoroughly warmed-up by being allowed to run continuously for at least half-an-hour before the clock was used. We therefore recommend that the clutch be operated about 10 times before the apparatus is put into use. Furthermore, if an interval of 10 or more minutes elapses between measurements (as in our case between the measurements at Settings A and B), the clutch should again be 'warmed-up.'

(4) After 'warming-up,' the percentage of variability is about 0.5% for both of the Standard Time clocks at intervals of approximately 2/3 sec. (Setting A). This degree of constancy is adequate to most of the demands of psychological research. At shorter intervals the clocks are more variable. At intervals of approximately 1/7 sec. (Setting B), the large model (SWC-1) varies between 2-3%, the small model (SC-1) between 1-1.5%. This degree of constancy is adequate for class-room demonstrations and for general laboratory work, but it does not suffice for research.

(5) The small clock is more constant than the large. At long intervals the difference is not great, though it usually is in favor of the smaller clock, but at short intervals the variability of the small clock is about half that of the large.

These findings, the greater relative variability of the clocks at short than at long intervals and the greater absolute and relative variability of the large clock, point again to the clutch as the source of the variable error. The deviations in the action of the clutch would affect the measurements of short intervals proportionatly more than those of long. Again, the deviations incident to overcoming the greater inertia of the hands of the large clock, since a greater mass is involved, would lead to a greater absolute, as well as relative, variability of the measurements with the large clock than with the small.

(6) No two of the chronoscopes agree in their measurements of the intervals used in these tests. The values obtained from the Cornell chronoscope fall between those obtained from the Standard Time clocks. At Setting A the average of the last 90 measurements made with the large model (SWC-1) was 690.88 ± 3.82σ; with the small model (SC-1), 661.45 ± 2.84σ; and of the 10 measurements made with the Cornell chronoscope, 676.4 ± 3.2σ. The large model is about 2% (14.48σ) slower and the small model about 2% (14.94σ) faster than the Cornell chronoscope. The difference between the Standard Time clocks is 29.42σ, or 4.4% of the average of the smaller. At Setting B, the average of the last 90 measurements is 132.55 ± 3.97σ and 116.66 ± 1.74σ for the large and small clocks respectively. The average of the Cornell chronoscope was 130.00 ± 1.00—about 2% (2.55σ)

faster than the large Standard model, and about 10% (13.34σ) slower than the small model. The difference between the Standard Time models is 15.89σ or about 13% of the average of the smaller clock.

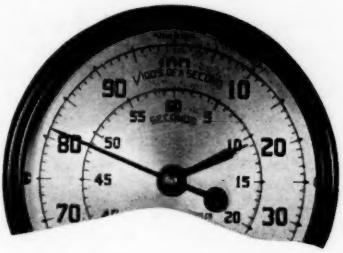

FIG. 1. DIAL OF MODEL SC-1

These results simply mean that every one of these clocks has its own constant error—and the conclusion carries over to every chronoscope whether or not it is run by a synchronous motor. Because the impulses of the current with which a clock is powered are constant and accurate there is no guarantee that the measurements made with the clock are either constant or accurate. As we have seen, they are neither. The constant error of the chronoscopes before us is probably due to differences in the friction of the clock-gears and also to differences in the inertia and slip of the clutch incident to the starting, rotating, and stopping of the clock-hands.

The size of the constant error is different for different clocks and it varies for a given clock with the length of the temporal interval to be measured—the greater the interval the smaller proportionately is the constant error. For demonstrations and routine work in the elementary laboratory in psychology, the constant error is not large enough seriously to affect the results. It need not, for such uses, be determined. It should be determined, however, either by the chronographic[5] or galvanometric[6] methods, whenever a high degree of accuracy is desired.

[5] For a description of the chronographic method of calibrating a chronoscope see Wundt, *op. cit.*, 383 ff.

[6] For the method of calibrating a chronoscope with a ballistic galvanometer see G. Kreezer, J. D. Coakley, and K. M. Dallenbach, A thyratron chronaximeter: Its operation and calibration, this JOURNAL, 46, 1934, 645 f.

APPARATUS NOTES

A Method of Calibrating Chronoscopes

The method of calibrating chronoscopes described here is an application of the principal of the stroboscope. It was developed and tested on a Dunlap (Johns Hopkins) chronoscope that was operated by D.C. interrupted by a 50 d.v. tuning fork, but it may be used on any chronoscope having an indicator or hand that rotates at a uniform speed. The method cannot, however, be used on chronoscopes operated by A.C., as the accuracy of the calibration depends itself upon the accuracy of the available A.C.—which may be checked directly by means of a frequency meter.

Our procedure is as follows. Divide a white cardboard disk of about 5 in. in diam., into 48 equal segments, and paint alternate segments black. Remove the indicator or dial hand and put this segmented disk in its place. When the chronoscope is operated this disk will rotate at the speed of the armature—at the same speed of the hand were it in its place. Illuminate the disk, in a semi-darkened room, by means of a neon bulb. If the chronoscope is operating at the correct speed, *i.e.* 5 r.p.sec., the black and white segments will appear to stand still. If they appear to rotate slowly in either direction, the chronoscope is running too fast or slow, and adjustments should be made in the mechanism of the chronoscope until apparent movement has ceased. If it is not possible to do this, or if the problem is one of calibrating measurements already made, the constant error of the chronoscope may be calculated as follows: Measure the amount of forward or backward movement in 10 sec., and then apply the following formula, $a/b = x$, in which a is the number of segments lost or gained in 1 sec., b is the number of rotations made per sec. by the dial-hand, and x is the gain or loss in seconds. For example: The Dunlap chronoscope has a 10-pole synchronous motor which revolves 5 times a second when a 50-d.v. tuning fork is used. If the black segments move forward one step (*i.e.* the width of one segment) in 10 sec., the constant error of the chronoscope would be $0.1 \div 5$ or 0.02 sec.

A disk with 24 uniformly spaced holes punched in around the circumference may be used in place of the one with white and black segments. The number of holes or segments required varies inversely with the speed of the apparatus and directly with the number of pulsations of the available A.C. current. The number required may be calculated by dividing the number of revolutions made per sec. by the chronoscope to be calibrated (5 in our case) into the number of pulsations per sec. of the available A.C. current (120 in case a 60 cycle current is used).

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LeVerne Johnson
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NOTES AND DISCUSSIONS

Concerning Physical Analogies in Psychology

However paradoxical it may seem, it is still true that psychology has for centuries struggled in vain to become a science because it attempted to pattern itself upon physics. The writer briefly reviews this apparently inconsistent circumstance as a background for considering one of the latest attempts to fashion psychological data upon the procrustean bed of physics.

(1) Physics as the model science. Despite all the radical changes that from time to time have been required in the fundamental concepts[1] and implications[2] of physics—even since its modern development in the 17th century—scientists generally, and notably psychologists, regard it not only as the science par excellence, but also the model for all other sciences.

There is, to be sure, something basically correct in this attitude, since it was physics (astronomy) as an organized science that historically first revolted against the mediaeval worship of speculative ideas based upon the limited observations of ancient times. Probably because of the nature of its subject-matter, physics (astronomy) was able to establish itself upon the only solid scientific foundation; namely, recorded observation.

Historically speaking, at any rate, all scientists must follow physics in proceeding upon a basis of observation and, whenever possible, experimentation. But surely this does not mean following physics blindly. For instance, the psychologist must not overlook the fact that physics in its very inception violated the law of observation to make most of natural phenomena into psychic substance—that is, to deny their existence as observable phenomena. By so doing physics asserted its faith in mystic essences. Under the inevitable influence of the culture of the time, such titans of science as Galileo, Boyle, and Descartes divided nature into primary and secondary qualities. It is undeniable that physics became a science while continuing the scholastic tradition of two worlds, and further that all dualisms in psychology go back to and perpetuate this tradition. Despite their continued mediaeval way of thinking, physicists nevertheless became effective scientists because they actually worked with observable phenomena—happenings with which they could interact—at least to the extent of expressing them in terms of Euclidean geometry. Because they set half of nature aside (including secondary qualities and psychological events) as non-natural, they put psychology into a hopeless situation for more than three centuries.

While it is not feasible to enumerate all the disadvantages arising out of this early dichotomy of the world, we cannot refrain from pointing out that psychology owes to this dualism (1) the mind-body nemesis (which has pursued us ever since), (2) the conception that consciousness (immediate experience) is more directly known than the facts of physics, (3) the creation of invalid psychological abstractions (minds, faculties, sensations, intelligence, instincts) as substitutes for concrete phenomena

[1] Action at a distance, constancy of mass, ether, proportionality of force and acceleration, particle mechanics, exclusively corpuscular or wave light, etc.

[2] Nature a mechanism; indeterminancy establishes free will; physics reduces to sensations; universe is ultimately material; God, a geometer.

(behavior, interactions), (4) the transformation of psychological data into other kinds (brain processes), (5) the explanation of psychological phenomena in terms of their physiological portions or phases, and (6) the misinterpretation of the crude, essentially psychological data by means of various types of analogical concept-borrowing.

Probably the most serious consequence of the dualistic influence upon psychology has been its inability to develop an autonomous, natural-science position, though obviously it has its own data and hence must have its own technique and descriptions. We cannot overstress the point that observers can start with the records of actual psychological interactions (perceiving, remembering, etc.) and build up a definite science, interrelated with all the other members of the scientific field, and yet independent.

Unfortunately, psychologists still think that following in the footsteps of physics means taking over certain general ideas or particular techniques. They believe that their discipline can attain full scientific maturity merely by measuring, experimenting, or quantifying, whether or not these processes are applicable to the phenomena dealt with or to the conditions of their observation. At the least, this belief overlooks the fact that what is to be done with data must be contingent upon their nature. Psychology therefore should follow physics in but one thing; namely, in building up a scientific structure from observable phenomena. Surely the psychologist must seek to avoid such cardinal errors as discarding happenings because they are difficult to deal with, reducing phenomena to mathematical relations, or interpreting events as something else, even though this has been done by physical scientists.

As if to provide us with a basis for a judicious attitude toward the conceptions of physics and physicists, Mach reviews the influence of preconceived notions upon the ideology of physical science.[3] Recall that the great heroes of modern science—Copernicus, Kepler, Galileo, Leibnitz, Maupertuis, Newton, Euler, the Bernoullis's, etc.—have been more than tainted with the odium theologicum. Even more important, recall that conceptions which so completely dominate modern physics as the calculus of variation, the constancy of the quantity of motion, least action, and the indestructibility of work or energy all arose under the influence of theological ideas.

From these references to physics we wish only to draw the conclusion that science consists essentially of a structure of which the materials are recorded events. Since it is inevitable that men of various experiences and intellectual backgrounds build up such scientific structures we must always ask: What postulates do they use and what foundations do they build upon? While it is impossible for scientific workers to avoid completely the cultural atmosphere of the time, it is a cardinal scientific principle to be eternally vigilant concerning the proportion of cultural and observational materials in any scientific construction. Here we are reminded of Newton's famous dictum *Hypotheses non fingo* in the concluding scholium of the Principia. In this century, obviously, we should be beyond the cultural influences of earlier periods and of most humanistic circumstances extraneous to science; but we have still to guard ourselves against the temptation of substituting the findings and results of other sciences for the kind of happenings that belong to our particular scientific field. As the writer has pointed out,[4] psychologists have done irreparable injury to

[3] Ernst Mach, *The Science of Mechanics*, 1907, 1-605.

[4] J. R. Kantor, Can the psychophysical experiment reconcile introspectionists and objectivists? this JOURNAL, 33, 1922, 481-510.

their science by basing it upon the abstractions of physics (suitable enough for that science) instead of beginning with non-traditional observations of psychological events.

(2) Interactional loci and physical fields. We come now to the most recent attempt of psychologists to imitate physics; namely, the gestaltic borrowing of the field-conception. In this case as in others there is something exceedingly appealing in the borrowed concept. Just as the principle of association appeared to introduce a mental counterpart to physical gravitation, and associationistic analysis paralleled atomic chemistry, so field-structures appear as valuable tools for the solution of psychological problems. Unfortunately in the present case, as in many others throughout the history of psychology, superficially attractive principles have not worked out to satisfactory conclusions because they were brought into a domain in which they were not really at home.

(a) Fields as loci of interactions. Before proceeding with the examination of the gestaltic concept of field we must state that in one sense a concept of fields is not only acceptable but necessary in every science. If by field is meant a locus of interaction of the factors in an event, it is an indispensable feature of every scientific description. This is true for two reasons. In the first place, every scientific phenomenon represents something arbitrarily though not capriciously isolated from a capacious manifold. A spatio-temporal frame of reference illustrates such an isolated entity. In the second place, no event can be described in fewer terms than two variables. Now the relationship of these two or more isolated variables, whether dimensions, functions, or any other kind, constitutes a field. We take it that the principle here referred to is that which differentiates between the Galilean and Aristotelian modes of thought.[8] This principle distinguishes between the idea, say, that a body falls because it has that nature—whether general (being a moving thing) or specific (in the sense of being impelled by its nature to move toward the center of the earth)—and Galileo's statement of the proportionality of velocity and time as measured in a frame in which two objects are interacting.

In the sense outlined psychology can, nay, must, operate with a field-concept, but this fact does not allow us in any way to overlook the differences between different types of phenomena and the variations in the fields required to study them. It is incontrovertible that psychology can establish itself as a science by regarding its data as interactions of organisms with other organisms and objects. Psychology, however, must steer just as wide from all implications that that which guides or determines the way the organism interacts with things consists of physical strains and stresses or mystical physiological conditions in the organism as from mental forces or powers (conation, instincts, urges) controlling the actions of organisms. Obviously this does not mean that we overlook the bonds that unite all the sciences. Accordingly, since psychology is interrelated with science in general, the former must not only be in touch with what appears as the better type of science, but also in consonance with it. For example, since it is true that physics is a more general science than psychology—because all psychological phenomena are also physical, while physical events are not psychological—then, if it seems better to replace Newton's gravitation—with its implication of action at a distance—by Einstein's gravitational field, in which the interrelations of bodies define the spatio-temporal factors, the psychologist will do well to adhere to this conception.

[8] Kantor, *Principles of Psychology,* 1924, i-xix.

At the same time we cannot disregard the great loss to psychology in overlooking that it is an autonomous science with data and principles of its own. True enough, such data never can clash with the laws of physics, but they are certainly different from those of physics. This point need not be argued, yet how full of disserviceable analogies is the history of psychology! In former times psychology concerned itself with psychic atoms (sensations) presumed to be the psychological counterparts of chemical atomism. Today, we find Koffka asking: "Can we introduce the field-concept into psychology, meaning by it a system of stresses and strains which will determine real behavior?"[6] Of course, he cannot answer with an unqualified affirmative and so the only result of trying to make psychology a branch of physics is to repeat once more the process of putting into psychology secondary, *i.e.* phenomenological materials by differentiating between real and phenomenological organisms and behavior.[7]

(b) Gestaltic psychodynamics. As a concrete example of how the Gestaltist employs fields we may refer to Lewin who attempts to describe the development and behavior of children as the result of the organization of forces in a field (environment).[8] He writes: "To determine not only which locomotions (paths) are possible but which of the locomotions will occur at a given moment one has to use the concept of force."[9] He adds that three properties define a force: (1) direction, (2) strength, and (3) point of application. In a series of diagrams Lewin indicates by means of a suitably applied conventional arrow-symbol for a mathematical vector that the organism is being directed at a certain strength toward an object. The physical analogy is completed by the assertion that "the real locomotion must occur in every case according to the direction and the strength of the resultant momentary forces," and "in any case of locomotion there exists a resultant of forces in its direction."[10] The sources of the forces lie in positive = approaching, or negative = withdrawing valences of objects. Valences (*Aufforderungscharaktere*), Lewin believes, include driving and restraining forces.

To illustrate the direction of the field-forces, Lewin cites the action of a 6-mo.-old infant stretching arms, legs, and head toward a rattle or spoonful of porridge. Again, the child who wants to get a piece of chocolate located on the other side of a bench has difficulty not because of the length of the detour, but because the "initial direction of the appropriate route does not agree with that of the vector from the valence."[11] Also the difficulties of the young child in sitting down on a chair are due to the fact that he must turn around to do so and thus "execute a movement opposed to the direction of the field force."

Setting aside for the moment the question whether in such cases as getting porridge or chocolate or sitting down on a bench one can reduce the behavior to a locomotion, as though one were merely describing the trajectory of a particle, the query arises whether the words valence and field-forces are anything but metaphorical terms. Do these terms refer to psychological events in a scientific manner or are they carried over as a matter of personal preference from a domain of altogether different phenomena? If the latter, there can be no issue concerning relevance, accuracy, or validity.

That Lewin by the use of these terms does not intend to reduce psychological

[6] Kurt Koffka, *Principles of Gestalt Psychology*, 1935, 42.
[7] *Ibid.*, 40 ff.
[8] Kurt Lewin, *A Dynamic Theory of Personality*, 1935, 1-286.
[9] *Ibid.*, 80. [10] *Ibid.*, 81. [11] *Ibid.*, 82.

phenomena to physical events is clear beyond doubt. In the first place, direction in the psychological field is not to be identified with physical direction[12] despite much emphasis upon topological concepts. Though Lewin says: "The topological description determines [sic] which points the different paths lead to and which regions these paths cross."[13] He also says: "When the child fetches a tool or applies to the experimenter for help, the action does not mean, even when it involves a physical movement in a direction opposite to the goal, a turning away from the goal but an approach to it."[14]

In the second place, Lewin asserts that the strength of the valences depends upon factors internal to the acting organism and especially momentary needs as well as age, etc. Then why all this physical paraphernalia (words)? How describe activities involving needs, purposes, wants, and desires in terms of vectors or fields of force? Is it not a fundamental contradiction to say on the one hand that the behavior of the organism is determined by a vector from a valence and on the other by conditions in the organism? Should not the behavioral event be described as an interaction between the organism and object? As Lewin elsewhere properly points out,[15] to understand or predict the psychological behavior B of an organism one must take into account the person P and the psychological environment E. $B = f(PE)$. A description of a psychological event, then, cannot be made in terms of the topological structure of a situation, if that implies a static organization of factors, as is true of physical dynamic and topological systems. Psychological phenomena involve cumulative factors along a developmental space-time continuum. More, psychological descriptions call for concrete series of happenings in the interactions of given individuals with particular objects in specific situations. For example, the child's difficulties with the seat may require description in terms of his failure to build up response configurations coördinating with particular stimulus-functions of the seat as a whole and as parts.

The writer submits that Lewin's emphasis upon field organization and other physical analogies represents an unsuccessful attempt to attain a genuine interactional view. This suggestion is firmly supported by his constant, confusing shift of position from a psychological to a physical field-statement. Since such a statement as: "Thus, history, as the child has experienced it, is also a psychologically essential constituent of the things of the environment"[16] constitutes an approach to an authentic psychological interactionism, we propose in the remainder of this paper to compare the physical analogies of the gestaltist with the interactional theory.

(3) Psychological interactions vs. field-forces. As compared with physical studies, psychological investigations presuppose that each event inevitably involves the following phases: (1) a particular type of interaction, (2) its momentary origin, (3) its intimate character, and (4) its historical possibility. In physics the tendency is, of course, to reduce all phenomena to abstract and general (mathematical) terms. How many physicists have entertained the hope of reducing all physical phenomena to the comparatively simple laws of mechanics! Psychological data and problems demand that strictest attention be given to the particularities of individual events, though to be sure specific workers may stress whatever features interest them the most.

(a) What psychological organisms do. Psychological interactions and analogical field-forces can be excellently compared by considering their basic descriptive

[12] *Ibid.*, 84.
[13] *Ibid.*, 79.
[14] *Ibid.*, 82.
[15] *Ibid.*, 80.
[16] *Ibid.*, 77.

foundations. Says Brown: "The central idea in a field-theory is that the behavior of an object within a field is determined by field-structure or by the spatio-temporal configuration of the energy within the field."[17] How apply this to psychological phenomena—for example, *A*'s blushing when *B* utters an insulting epithet? In isolation, the statement may be well taken as descriptive of the motion of two balls on a billiard table. But how significantly can one describe affective behavior in these terms? Are we dealing with energy distribution? What is the difference between *B*'s energy expenditure when uttering a compliment or an epithet? To convert (if possible) what is happening here to energy terms raises the question: Can complex phenomena be reduced to the simplified abstractions of physics or geometry? Here, as elsewhere, the field-force proponents do not really mean what they say, for Brown[18] distinctly rejects even such physicalism as that of Watson or Weiss. Also Lewin admits that there are cognitive processes which change the structures of a field.[19] They merely mean to get away from the idea that what objects do is based on their class or structure.

Compare with such hypothetical field-forces the description of psychological phenomena as specific configurations—the blushing act of insult or the striking act which follows—coördinated with a stimulating property of another action (linguistic act of *B*). From whatever standpoint we take it, adequacy, concreteness, exactitude, or significance of description, the interactional construction is highly informative concerning why *A* is insulted by *B*'s and not *C*'s action, why this particular and not some other act of *B* is insulting, etc. Can such features be comprised within the boundaries of a spatio-temporal field unless a sharp differentiation is made between a physico-geometric and a genuine psychological field? Clearly not. We therefore submit that the use of the term psychodynamics by the field-force proponents is not enough.

At most, the discussion of field-forces can only apply to a momentary descriptive procedure. We may grant that the vector-valence diagrams are extremely useful in describing the pathway traversed by a child stimulated to move toward the table to obtain a bit of chocolate, but, as we have said, that is a very minute phase of the total phenomenon. Be it admitted also that in some cases the specification of pathways may indicate very important phases of the events described, as, for example, in the study of rat-maze behavior. Lewin is, of course, thoroughly aware of this, for he writes that, in psychology as in every other science, for the explanation or prediction of events we must know: (1) "the general laws of particular events" and (2) "the momentary constitution of the situation."[20] Furthermore he asserts that the vector is not a concept of topology[21] and also protests emphatically that psychologists must distinguish sharply between physical and psychobiological fields.[22] The latter apparently is constituted of the psychological environment and the structure of the living being.[23] In the final analysis, however, Lewin comes back to a physical concept.

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[17] J. F. Brown, *The Mathematical Conceptions Underlying the Theory of Psychological and Social Fields*, 1935, 10.
[18] *Ibid.*, 12.
[19] Lewin, Vectors, cognitive processes and Mr. Tolman's criticism, *J. Gen. Psychol.*, 8, 1933, 337.
[20] *Ibid.*, 319.
[21] *Ibid.*, 330.
[22] *Ibid.*, 322.
[23] *Ibid.*, 320.

Says he: "It is true that we use the idea of *resultant of forces,* and I think that only if one uses the idea of the resultant the concept of forces and the concept of directions can be useful in psychology. Above all, it is impossible to use the important concept of *equilibrium, without presupposing the idea of a resultant of forces.* Hence the equilibrium is only a particular case of such a resultant. It may be that in psychology we shall yet have a theorem like that of the parallelogram of forces in physics."[24]

That Lewin's concepts revolve around a physical center is all the more remarkable when we notice that unlike most psychologists he appreciates the need for considering the specific and individual. Thus he vigorously criticizes the intense absorption of psychologists with statistics and averages with the consequent masking of the essential details of psychological phenomena.[25] The writer believes that Lewin's constant resort to irreconcilable views can be obviated by distinguishing between the different types of fields, and pushing forward from the physically analogous field to one of genuine psychological interaction.

To illustrate the psychologist's specific type of interaction we must ascertain whether the child's behavior of approaching the table to get a piece of chocolate is an interaction with chocolate or merely with something which stimulates him because it is on the table or has just been placed there. Other questions are: Is there curiosity involved or desire to obtain something known to be good? In general we may analyze out of a psychological event an integrated set of behavior phenomena included in the conventional list. It is safe to say there would always be perceptual, habitual, memorial, affective, and other types.

(b) What originates behavior? However great may be the contrast between physical statics and dynamics (kinetics), the latter, when analogously carried over into psychology, becomes completely static. In the final analysis physics deals with the behavior of things, whereas psychological phenomena are fundamentally events—interrelations of functions. Again, physical dynamics always implies a status quo, even if a present status is derived from a previous one. By contrast, a psychological event constitutes a genuinely durative happening. Now, if in addition we consider the utter simplification of kinetics from the standpoint of the innumerable factors of a psychological situation, we can easily discern the insufficiency of physical dynamic fields for psychological description. We are concerned here, however, with the question why any particular behavior begins. Organisms are always active. They are constantly surrounded by all sorts of objects and conditions with which they interact. To say that the "causes of events is the relationship between the parts of a situation as dynamical facts"[26] is to refer at most to some phases of events after they have happened and not to offer an account of their origin or a basis for their prediction.

Indeed it is to the credit of Lewin that he places so much emphasis upon "the momentary constitution of the situation" in order to do away with unpalatable mechanisms (such as mysterious motivations) behind the observed facts. Granting that a psychological phenomenon cannot be described in physical terms, the question arises whether satisfactory psychological materials are available for the purpose.

(4) Valences vs. stimulus-functions. The closest approximation to an originating

[24] *Ibid.,* 34.
[25] Lewin, *A Dynamic Theory of Personality,* 1935, 16, 20, 67.
[26] Lewin, *op. cit., J. Gen. Psychol.,* 8, 1933, 319.

factor that the gestaltist offers is valence. An organism moves toward an object because of its positive valence, away from it, if the valence is negative.[27]

Despite Lewin's physical nomenclature, it is plain that any plausibility or validity accruing to the term valence derives from the fact that he is actually dealing with concrete facts of interaction. How far can valence (sheer attraction and repulsion) go in view of Lewin's observation that objects variously attract the child to eating, climbing, grasping, manipulating, sucking, raging, etc.[28] Consider, too, that while the interactions of children with objects are complex enough, adult interactions with the same things are still more so. Moreover, the same things attract and repel and in general are behaviorally interconnected in many ways with a particular person.

Once more we reiterate that throughout Lewin's work he makes sufficient allowance for concrete interactional factors which make his physical analogies superfluous. We are not, however, interested merely in dispensing with inept terms, rather, arriving at a coherent and serviceable statement concerning psychological phenomena.

Must we not for this purpose regard the present psychological happening as a phase in a historically continuative process? Objects have no power of attraction or repulsion except insofar as they have taken on stimulus-functions in previous interrelationships with the organism, which in turn has built up coördinate response systems or configurations. A truly workable psychodynamics can be thought of only in terms of a spatio-temporal reference frame involving this historical factor. The writer has elsewhere[29] discussed fully the various conditions under which these response-configurations and stimulus-functions are coördinated, as well as the transfer of stimulus-functions from one object to another. He proposes that it is only by means of such a psychodynamic conception that psychologists can get rid of the physiological idea of stimulus and response. Certainly Lewin tends in this direction, when he says: "The perception of an object or event can thus cause the formation of a definite tense psychical system."[30] Nevertheless, he constantly shifts back and forth between psychical and physically analogous energies.

In his psychodynamics Lewin overlooks the fact that the psychologist is dealing with highly evolved organisms existing and acting in complicated civilizational situations. Accordingly when we choose our illustrations not from a single locomotor-situation, but from an ordinary interaction between persons, we can observe how a remark-behavior of *A* has been endowed with the stimulus-function of referring *B* to some object or event. In such a situation it is evident that *A*'s behavior, stimulated by the sight of *X*, in turn stimulates *B*'s behavior. However easily one may interpret such phenomena in terms of rearrangement of energies in systems, the question arises whether the adequacy and significance criteria of description are met.

Gestaltists are generally opposed to the stimulus and response form of psychological description. Why? There are several answers which afford them fair justification for their aversion. Quite rightly they object to the physiological conception of stimulus, since obviously it is impossible to regard a psychological response as a release of energy set off by a physiological stimulus. Again they observe what is

[27] Lewin, *op. cit., J. Gen. Psychol.*, 8, 1933, 333; *A Dynamic Theory of Personality*, 1935, 81.
[28] Lewin, *op. cit.*, 1935, 77.
[29] Kantor, *Principles of Psychology*, 1924, 1-473; *A Survey of the Science of Psychology*, 1933, 1-564.
[30] Lewin, *op. cit.*, 1935, 51.

now a well-known fact, that a form of physical (radiant or mechanical) energy cannot be correlated with the appreciation or discrimination of a color or some actual object. Conventional stimuli as dealt with by both behaviorists and existentialists constitute atomistic and unorganized abstractions which the Gestaltists are entirely correct in rejecting.

Furthermore, the Gestaltists are justified in renouncing the inept idea that a stimulus is the cause of a response, since this idea harks back to an unsatisfactory disjunction of an event. But we propose that the remedy here is not to involve ourselves with a simplified arrangement of a physical field, but rather to assimilate the idea of a durative and historical event. To object to a wrong conception of stimulus and response is a poor basis for disregarding a more useful and acceptable notion. The Gestaltists are undoubtedly overlooking the most recent developments in the interpretation of these descriptive tools. We urge that to look upon responses as definite behavior or response-configurations coördinated with stimulus-functions of objects and events not only offers a serviceable technique for describing the origin and transpiration of psychological happenings, but also helps to avoid interpretatively harmful physical analogies.[31]

(5) Conflicts and tensions. Lewin's ideas concerning conflicts and tensions illustrate well the opposition between psychological interactions and field-forces. In his characteristically metaphorical vein he defines a conflict as the opposition of approximately equally strong field-forces. As illustrations he gives the following cases: (1) "The child stands between two positive valences. He has to choose perhaps between going on a picnic and playing with his comrades . . . (2) the child faces something that has simultaneously both a positive and a negative valence. He wants, for example, to climb a tree, but is afraid . . . (3) the child stands between two negative valences, for example, when it is sought by threat of punishment to move a child to do a task he does not want to do."[32] Granting that Lewin has successfully drawn diagrams to represent what he calls conflicts—though it is difficult to see how one can speak of unpolarized positive and negative forces as in (2)—do the putative forces they represent compete even feebly with reactional stimulus- and response-functions as descriptive tools for handling the activities of the child?

As over against the equilibrium of forces we suggest the consideration of various reactional configurations nameable as daring, fearing, liking, disliking, built up by the organism in interaction with objects, persons, and their characteristics, some of which may compete with others under certain circumstances.

Similarly with tension. Here restlessness and random activities of children are presumed to exemplify the opposition of field-forces on the individual. There are several difficulties here. First, as the "opposition of field-forces in every direction"[33] tension constitutes a static equilibrium, but, on the other hand, it is the source of all sorts of activities. The physical analogy introduces a serious contradiction. Again, would it not be more in line with observation to say that such infantile behavior consists of gross uncoördinated activities which have not yet become integrated with specific stimulus-functions of objects, as the present writer has pointed out?[34] Finally,

[31] Kantor, In defense of stimulus-response psychology, *Psychol. Rev.*, 40, 1933, 324-336.

[32] Lewin, *op. cit.*, 1935, 89 ff. [33] *Ibid.*, 94.

[34] Kantor, *Principles of Psychology*, 1924, 146 ff.; *A Survey of the Science of Psychology*, 1933, 85 ff.

to regard tension, whether well or ill-defined, as a source of all sorts of activities is to run pretty close to the brink of making psychological activity into self-caused behavior unrelated in a most crucial manner with interactional objects.

(6) Field structure and setting. It is obvious that the complete description of a psychological event requires taking into account something more than the past and present activities of the organism in interaction with stimulus-objects. There are the surrounding conditions. For example, the child finds both chocolate and fruit on the table. Because of a more potent stimulus-function with which he has previously endowed the chocolate he would ordinarily take it, but in the presence of the nurse, who frowns upon chocolate as less hygienic, he takes the fruit. A similar type of setting is found in the barriers which Lewin has made so much of in his field-force diagrams. Now the question arises whether it is preferable to follow physical analogies and to describe such situations in terms of field-structures, or to take into account the actual effect of settings upon the original building up of stimulus-response coördinations and their later operation under the influence of such psychological auspices.

Elsewhere[35] the writer has discussed the nature of these settings and their effects upon either the acting individual, the stimulus-object, or the total interaction. The writer submits that the theory of settings avoids the simple paradox of a perceived barrier interfering with actions in the direction of field-forces (a paradox not resolved by regarding the forces as merely psychic)[36] or the intensification of the paradox provided by another statement to the effect that "the barred character of the barrier increases if the valence behind the barrier increases."[37] On the one hand, the barrier can destroy valences which are the determiners of the direction of the behavior, but, on the other, the barrier-character can be increased by the increase in valence. Surely these assertions call for a further analysis of interbehavioral events to make them intellectually palatable. For one thing, surrounding objects may be facilitators of interactions as well as interferences. The general efficacy of such factors in interactional situations is what gave the psychologist so much concern in dealing with cues of all sorts.

As we have indicated above, it is not possible to describe any other than a sheer locomotor activity in terms of an organism's translocation in space. All other forms of interaction are exceedingly complex events. There are, then, a number of factors which determine the character of such events as they transpire. First, the intrinsic configuration of the organism's behavior can be traced back to its development while the organism was in contact with particular objects and conditions—in other words, to the individual's reactional biography. Such contacts and conditions determine whether it is a manipulative, a withdrawal, an effective, a reflective, a verbal action, or some other. Again, what kind of behavior the individual develops at any particular time depends upon his current psychological nature, which is nothing other than behavior equipment previously developed. Such equipment obviously reflects the anthropological conditions under which the individual lives. Naturally all developments of equipment depend upon the organism's present health and vigor. Next we may mention the stimulating circumstances as we have treated them under the heading

[35] Kantor, *Principles of Psychology*, 1924, 55 f.; *A Survey of the Science of Psychology*, 1933, 34 f.
[36] Lewin, *op. cit.*, 1935, 89.
[37] Lewin, *op. cit.*, *J. Gen. Psychol.*, 8, 1933, 332.

of behavior origins, and here, too, the organism's hygienic conditions at the time must be considered. Finally, the setting or behavior environment must be included.

The same general set of conditions determine also the intensity, velocity, tempo, and rhythm of the individual's behavior, as well as his willingness and eagerness of performance. How clearly different is such a handling of a psychological event from any physical or topological technique! Moreover, an interbehavioral description not only is scientific, but likewise makes room for all the necessary descriptive specificities that psychological phenomena require.

Conclusion. In this paper we have attempted to show that there are two contrasting versions of the field-concept in psychology. One is the gestaltic field of physical analogies, the other the essentially psychological field of interbehavior (organismic) events. Since we assume that the latter satisfies better the criteria of objective psychology, we propose that the Gestaltists do not reach their psychological goal because they retain so many traditional tendencies in their thinking.

By far the most important obstacle of the Gestaltist is his inability to get away from mentalism. Recall that the gestalt movement originated as a deviation from the atomistic view of the psychic. Granted that specific experimental results can be better interpreted on the gestalt rather than on the sensational basis, does this mean that a satisfactory mentalistic formulation of psychological phenomena is possible? Granted likewise that all the gestaltic criticism of isolated sensations is well grounded, there still remains the question of the existence (even as scientific conceptions) of organized mental qualities. What is required in psychological description is in no sense an occupation with either isolated or organized mental qualities, but the construction of propositions concerning the way organisms interact with actual objects. We suggest that no matter how much such interactions may vary in different individuals or how greatly varied are the interactions of a single individual with particular objects—as when a set of Müller-Lyer arrows are casually judged and actually measured—there is no room in psychology for mentalistic conceptions, whether or not disguised as phenomenology. The Gestaltist, however, cannot and does not conceal his commerce with egos, souls, selves, and other transcendent entities. In point of fact, physical paraphernalia merely constitute a means of screening this traffic.

Köhler[38] has unambiguously declared his adherence to direct experience and consciousness on the basis that while there is a physical object that starts off processes in the physical organisms of two persons who read a galvanometer, the fact that in each case the galvanometer is the result of those processes proves that there is a different galvanometer for each observer. Is this anything more than the confusion of intimacy and uniqueness of happening with 'experience' and 'consciousness'—in other words, with something other than an interaction of two things? When a stone strikes a mark, it is a different striking from that involving any other stone or the same stone in another event, however much we are able to generalize the striking. If difference in events constitutes consciousness, there is consciousness in a striking stone also.[39] As we have implied, it does not help matters when we use the nervous system of a galvanometer reader as a surrogate of a new factor beyond the phenomenon of interaction.

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[38] Wolfgang Köhler, *Gestalt Psychology*, 1929, 29.
[39] See Kantor, *op. cit.*, this JOURNAL, 33, 1922, 499-500.

Again, however closely the Gestaltists' emphasis upon total situations may approach the scientific conception of events and true fields, their laxness in accepting the individual evolution of mentality in the life of an organism leads to an unsatisfactory conception of dynamics. We can only conclude that physical analogies can never help psychology reach the goal of a natural science.

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THE MEANING OF 'DIFFICULTY'

Investigators who have considered the effect of practice on individual differences have usually encountered the fact of progressively slower improvement with increasing practice, and have customarily ascribed this to the increasing "difficulty" of the successive units.[1] A good statement of the relevance of this consideration to the problem is to be found in Stone.

> An inspection of such [learning] curves will tell us whether they draw together, fan out, cross, or remain parallel, but it cannot tell us anything about the magnitude of differences as a result of training, because we do not know how to equate for the increments of difficulty. The curves at the end of training may have drawn much closer together, but in reality they may be considered much farther apart. For example, if the curves of two individuals are 6 points apart on the vertical axis at the beginning of a training period and only 3 points apart at the end of training, it is possible to say that these individuals have drawn closer together; but the 3 points at the end may take much more ability to overcome them than the 6 points at the beginning.[2]

It is clear that a shift in the meanings of terms has occurred between the beginning and end of such an inquiry; the author's original objective was to determine whether the differences (in performance) increase or decrease as a result of practice—but having (hypothetically) determined that they decrease, he is not content with this unequivocal result, but rejects it because he now wishes to know something else, viz., the fate of differences in "ability." This greatly complicates the problem, however, and introduces a spurious element; for "ability" to do a task is not known, and can hardly be known, except through actual performance of the task. Thus instead of a clear-cut determination of the trend of tangible differences, we are now confronted with the problem of determining either an intangible, unmeasurable entity or a mere functional transformation of our original measures.

Some writers have gone so far as to affirm that the successive increments of performance are not equal—a metaphor which, probably, most of them would probably admit to be untrue in the strict meaning of the words, but which they ordinarily write in such form as to appear literal to the unsophisticated student; this, it seems to us, is bound to confuse and not to clarify. The objective units, if properly chosen, are obviously equal; one second is like another second, one foot like another foot, one load of ten pounds like another load of the same weight, shape, and distance transported; one arithmetic problem, even, is like another arithmetic problem, if some intelligence is exercised to make it so. The inequality, in every case, concerns the proportion of *Ss* who succeed in performing certain specified operations upon these units under specified conditions. The explanation of decreasing improvement in

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[1] W. S. Hunter, Learning: IV. Experimental study of learning, *A Handbook of General Experimental Psychology*, 1934, 497-570.

[2] R. Stone, *The relation of training to the magnitude of individual differences*, Clark Univ.: Master's dissertation, 1928, 52.

terms of increasing difficulty appears to be a circular description of this fact; *i.e.* progressively fewer *S*s can perform the successive units of task because these units are such that they can be performed by progressively fewer *S*s; or the same *S* can perform progressively fewer units of task per unit of time because these units are such that progressively fewer of them can be performed per unit time.

The same problem appears again in the relation between test content and distribution of performance. That a frequency distribution of scores is skewed downward does not mean *per se* that the population is efficient; if we desire a more nearly normal distribution we have only to eliminate the "easier" items. In other words, when we choose items which more *S*s have been found to perform successfully, we shall find more *S*s (of the same sort) performing them successfully. Perhaps the best way of breaking the circle in this last problem is to choose items so as to make a stable standard population yield a normal distribution, and then to describe other population distributions with reference to this one.

The former (individual-difference) problem, which we have suggested above is probably spurious in the forms in which it is customarily discussed, has recently been studied by Anastasi[3] and by Ewert.[4] Anastasi's solution is the neater, and proceeds by the use of standard scores; if we have correctly understood her transformations, this is equivalent to rectifying the original (observed) approximately hyperbolic relationship between performance and time so that it shall be linear, and then comparing the slopes of these (derivative) linear functions. Ewert's solution is much more difficult to follow—it proceeds in terms of weights, derived from a consideration of the average number of trials (or errors) required to eliminate a trial (or error) at various parts of the range, and applied to the observed scores—but it appears to amount to a very similar rectification of a non-linear function into a linear one; the essential difference between them is that Anastasi's rectification is effected on a simple frequency basis—proportion of a population surpassing a given individual's performance—while Ewert's, a much more complicated matter, is ultimately a function of analysis of individual curves. Both studies, however, are open to the objection that in the welter of algebra the original objective is completely obscured, and the investigator emerges with a not very reliably established answer to a *different* problem—one having to do not with the effect of practice on individual differences in performance, but with individual differences in some remote and complicated derivative of performance scores; we say "a not very reliably established answer" because, presumably, an error is introduced with each transformation, and there is no reason to suppose that the successive errors are compensating.

We shall, however, give some attention to the concept of difficulty itself, with the object of determining whether the circle referred to can be broken, or whether, on the contrary, the term should be abandoned or replaced in the interest of clearer thinking.[5]

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[3] A. Anastasi, Practice and variability; a study in psychological method. *Psychol. Monog.*, 45, 1934 (no. 5), 1-55.

[4] H. Ewert, The effect of practice on individual differences when studied with measurements weighted for difficulty. *J. Gen. Psychol.*, 10, 1934, 249-285.

[5] We wish to exclude explicitly from the discussion the *subjective* meaning of "difficulty," as in the sentence, "The ascent to the summit was difficult," or "It was difficult to meet the man on a basis of equality." These refer chiefly to subjective phenomena (the first possibly also to considerations of energy expenditure and metabolism) and appear to belong to a different universe of discourse.

'Difficulty' appears to be in essence a dichotomous concept: a specified task can or cannot be performed. The exception that parts of it can be performed while others cannot seems to be apparent only, since it involves a change in the task originally specified—the new (sub-) task, again, can be performed or it cannot.

Why can a task not be performed? Evidently, because a complete causal chain does not exist. The transmission of energy through a physical system is impossible unless the system is (functionally) intact; *e.g.* a short-circuit or the removal of a coupling will prevent the passage of an electric current. Likewise a specified physiological energy transformation, like the passage of a nerve impulse, requires the presence of the necessary structures and the necessary properties in these structures. While we do not have the corresponding information about psychological phenomena of parallel order, there seems to be no objection to the analogy that the performance of a psychological task requires the presence of certain structures and relationships, *e.g.* mental multiplication requires the presence of associative bonds between pairs of numbers and their products; lacking the necessary links of the causal chain, the task is too "difficult," *i.e.* cannot be performed. Thus the dichotomy "can or cannot be performed" corresponds to the dichotomy "presence or absence of particular structures or properties" and in fact is a dichotomy precisely because there are no intermediate stages between presence and absence.

It would thus appear that the 'difficulty' of marginal units is a function of the rarity of the necessary structures and structure-organizations in the population; and this in turn is evidently to be thought of as a consequence of the circumstances which bring about Gaussian distributions in general, viz., the interaction, on a probability basis, of a large number of small approximately equal causes. Since in the absence of skewing sufficiently marked to be studied successfully these causes will ordinarily remain undetected, we are thrown back upon our first proposition: "The item is difficult" means "few people can do it, *i.e.* have the necessary equipment to do it"; and it means nothing further.

There is one apparent exception to the reasoning presented above. In terms of *individuals,* a task may sometimes seem to hover on the border between performance and non-performance, so that it is performed intermittently; it is then sometimes said to be "difficult" (particularly if there is also subjective distress). An analogy from the physical field is that of a 'laboring' engine, which 'skips' occasionally but does not stop; the explanation whereof seems to be that the factors usually adequate to the task are occasionally no longer so, but really do cease functioning; and that the continuance of the process as a whole is made possible by reinitiation of their functioning, at some moment when they are again adequate, by ordinarily secondary forces having different temporal characteristics, such as the inertia of the flywheel, which in fact has been introduced into the system for precisely this purpose. The full transferability of the analogy may be open to some question, but it seems possible to postulate a set of conditions wherein small alterations of ratio between task and requisite energy (by alterations of either or both) may cause frequent stoppages and re-startings initiated from outside the system most intimately concerned, with consequent over-excitation and reports of distress.

We may, then, conclude that the concept of difficulty (in its objective aspects) should for purposes of precise statement be renamed; and we suggest some such phrase as "incidence of successful performance" (under specified conditions) as relatively free from the possibility of misinterpretation through ascription of more or

less magical properties to units of the task itself; in certain special situations, it may be that reference to time or energy expenditure per unit of work would more adequately clarify description—thus an "easy" task, in these circumstances, would be one requiring relatively slight expenditures of time and energy.

Clark University RAYMOND ROYCE WILLOUGHBY

THE SIGNIFICANCE OF MATCHINGS WITH UNEQUAL SERIES

In the 1934 volume of this JOURNAL the writer published an article[1] (hereafter referred to in this note as "CM") on the significance of the number of correct matchings resulting from the arrangement of t items against t apposite items. He has subsequently published the solution of the more general case in which a series consisting of t items is arranged against a longer series consisting of u items, when t of the items in the longer series are true apposites of the items in the shorter series.[2] The purpose of this note is to furnish briefly and without proof the formulas necessary for the appraisal of the results of the method of correct matchings when the two series matched are of unequal length, thus supplementing the formulas of CM.

(1) The significance of s correct matchings by a single S. In CM it was shown that the probability that s or more correct matchings will result from a single random arrangement of t items against t apposite items is a function of t and s, and a table (Table II of CM) of this probability was presented for values of t and s. But when t items are arranged against u items ($t<u$), the probability of s or more correct matchings becomes a function of s, t, and u; and therefore no such simple tabulation is possible. In any case, however, this probability, $P_{(s \text{ or more})}$, can be computed from the following formula (remember that $0! = 1$):

$$
\begin{aligned}
P_{(s \text{ or more})} = \frac{t!}{u!}\Bigg\{ & \frac{(u-s)!}{(t-s)!}\left[\frac{1}{0!s!}\right] \\
& + \frac{(u-s-1)!}{(t-s-1)!}\left[\frac{1}{0!(s+1)!} - \frac{1}{1!s!}\right] \\
& + \frac{(u-s-2)!}{(t-s-2)!}\left[\frac{1}{0!(s+2)!} - \frac{1}{1!(s+1)!} + \frac{1}{2!s!}\right] \\
& + \cdots \\
& + \frac{(u-t)!}{0!}\left[\frac{1}{0!t!} - \frac{1}{1!(t-1)!} + \frac{1}{2!(t-2)!} - \cdots + (-1)^{t-s}\frac{1}{(t-s)!s!}\right]\Bigg\}. \qquad [1]
\end{aligned}
$$

Thus if an individual were to produce three correct matchings by arranging five items against seven items, we should have $s = 3$, $t = 5$, $u = 7$; and the statistical significance of this performance (the probability that so good a performance should result from chance alone) would be given by

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[1] D. W. Chapman, The statistics of the method of correct matchings, this JOURNAL, 46, 1934, 287-298. I take this opportunity of making the following corrections to the article:

P. 293, first equation: For $(1-1)$ *read* $(1-1)^m$.

P. 294, Table VI: For $\beta_2 = 1/n$ *read* $\beta_1 = 1/n$.

P. 294, line following Table VI: For $(\beta_1 < 1)$ *read* $(0 < \beta_1 < 1)$.

P. 296, line 1: For the mean of which was computed *read* the means of which were computed.

[2] D. W. Chapman, The generalized problem of correct matchings, *Ann. Math. Statist.*, 6, 1935, 85-95.

$$P_{(3\text{ or more})}=\frac{5!}{7!}\left\{\frac{4!}{2!}\left[\frac{1}{0!3!}\right]+\frac{3!}{1!}\left[\frac{1}{0!4!}-\frac{1}{1!3!}\right]+\frac{2!}{0!}\left[\frac{1}{0!5!}-\frac{1}{1!4!}+\frac{1}{2!3!}\right]\right\}$$
$$=.032.$$

(2) The significance of the mean number of correct matchings resulting from n independent trials. Let $P_{(m\text{ or more})}$ represent the probability that the mean number of correct matchings, m, achieved by n individuals independently arranging t items against u items should be equalled or exceeded by chance. In the article CM it was shown that this probability is the area under the curve of distribution of the mean number of matchings and above the lower boundary of the class-interval of m, and that this distribution is closely fitted by a Pearson Type III curve whose mean, standard deviation, and skewness are functions of n and independent of t for $t > 4$. A table of $P_{(s\text{ or more})}$ (Table VII, CM) was accordingly presented for the arguments m and certain values of n. But in the case that t items are arranged against u items, where $t < u$, the constants of the distribution become in addition functions of t and u, so that tabulation of $P_{(m\text{ or more})}$ becomes impracticable.

Now it is found that if we write

$$a=\frac{t}{u},\quad b=\frac{t-1}{u-1},\quad c=\frac{t-2}{u-2},$$

the mean of the distribution of the mean number of correct matchings is given by

$$M=a \qquad [2]$$

the standard deviation by

$$\sigma=\sqrt{\frac{1}{n}[a(1+b)-a^2]} \qquad [3]$$

and the skewness by

$$Sk=\sqrt{\frac{[a(1+3b+bc)-3a^2(1+b)+2a^3]^2}{n[a(1+b)-a^2]^3}}. \qquad [4]$$

If now n individuals produce together a mean number, m, of correct matchings corresponding to a total number, S, we have

$$m = S/n;$$

and since the total number of correct matchings produced by the group must be an integer, the next lower mean number, m', which could have resulted is given by

$$m' = (S - 1)/n.$$

The lower boundary of the class-interval of m is therefore the mean of m' and m, and is given by

$$\tfrac{1}{2}\,[(S-1)/n + (S/n)] = m - (\tfrac{1}{2}n). \qquad [5]$$

The procedure in assessing the significance of a given value of m therefore falls into the following five steps. (I) Compute the excess of the boundary given by [5] over the mean given by [2], and (II) divide this excess by the standard deviation [3], thus obtaining a standard deviate:

$$\text{Standard deviate} = \frac{\left(m - \frac{1}{2n}\right) - M}{\sigma} = \frac{m - \frac{1}{2n} - a}{\sqrt{\frac{1}{n}[a(1+b) - a^2]}}. \qquad [6]$$

(III) Compute the skewness by Formula [4]. (IV) In Salvosa's table[3] read the area under the Type III curve for this deviate and skewness, calling this area A. (V) Since A is the area from the lower end of the curve to the deviate, and we wish the area above the deviate, we then find $P_{(m \text{ or more})}$ from the relation

$$P_{(m \text{ or more})} = 1 - A.$$

For example, let 1.2 be the mean number of correct matchings per judge for a group of ten judges who have each arranged five items against eight other items. Then

$$m = 1.2, \quad n = 10, \quad t = 5, \quad u = 8,$$

and $a = t/u = .625$, $b = (t-1)/(u-1) = .571$, $c = (t-2)/(u-2) = .500$.

Substitution of these values in Formula [6] gives

$$\text{Standard deviate} = 2.16,$$

and substitution in Formula [4] gives

$$\text{Sk} = 0.36.$$

In Salvosa's table we find the area corresponding to this deviate and skewness to be .977, so that

$$P_{(m \text{ or more})} = 1.000 - .977 = .023,$$

which is to say that so good a performance should be expected by chance only about 23 times in 1,000.

One caution is necessary: Formula [4] requires, by its derivation, that the shorter of the two series to be matched contain not less than four items; and this limitation applies therefore to the method just outlined. Formula [1], however, is free from any restrictions on the magnitude of t, and may therefore be applied to all cases of matching by a single individual.

Harvard University DWIGHT W. CHAPMAN

"PSYCHOLOGICAL LOSS IN PARETICS AND SCHIZOPHRENICS:" A DISCUSSION

Hunt, in the article whose title is given in our heading, points to the "paradoxical" nature of his findings.[1] We would ask: "Why paradoxical?" Hunt seems to be considerably mystified by the results of his experiments, as to how they may be explained and interpreted. His data are perfectly consistent with well known and generally accepted psychological facts and principles and the outcome of his investigation is precisely such as we should *a priori* expect. The "psychological factors responsible"

[3] L. R. Salvosa, Tables of Pearson's Type III function, *Ann. Math. Statist.*, 1, 1930, 191-198.

[1] J. McV. Hunt, Psychological loss in paretics and schizophrenics, this JOURNAL, 47, 1935, 458.

are not far to seek. When Hunt states that "arithmetical progression was chosen because it presents a problem which contains within itself directions for its solution,"[2] he gives us the key to the solving of the problem, though he quite evidently does not realize that he had the key in his hands. When he says that the differences in performance between the two groups "appear to rest upon some fundamental difference in the psychological operations of schizophrenic and paretic,"[3] this is simply *ipsa res loquitur*, so to speak.

We may note at once that the number series present a purely formal aspect, devoid of particular content, and certainly with no immediate reference to objectivity. "To the schizophrenic the arithmetical problem is both understandable and solvable. By the paretic the problem is usually unsolved. Whether the latter outcome is due to the want of the paretics' resources for solving when comprehended remains unknown."[4] Why should it remain unknown? One of the instructions was: "If after that, *S* continued to inspect the progression without success, *E* suggested, 'Write down the differences.' "[5] A complete record of the differences in question should tell the story. A minor point may be mentioned here. Hunt says that the only progression the two badly deteriorated paretics could "solve" was 1, 2, 3, 4. . . . This was not solution in any real sense; this was merely the working of one of our oldest habit formations, the act of counting. Bleuler has remarked that in the case of the syphilitic psychoses "the psychic symptoms, both as to time and number, are lacunary."[6]

Contemporary knowledge concerning paresis justifies the view that it may be regarded *primarily* as a quantitative change, as a reduction of general capacity; needless to say, the quantitative alteration carries along with it inevitably a pathological transformation in respect to quality.

The group of reactions called schizophrenic are today best understood and interpreted from the functional point of view. Although there is diminution of quantity here, the changes must be looked upon as preëminently qualitative. Emotional distortion is of the very essence of the schizophrenias. Affective disturbances dislocate the mechanisms of thinking. The victim is abulic, will not face objectivity squarely. The raw materials of sense-perception are twisted and skewed in keeping with the feelings. Inhibition is a major symptom. There is everlasting need of a prod, a goad to action that bears any semblance to orderliness and logicality. There is the most intense preoccupation with the affective life.

In the schizophrenic the number series do not come into conflict with the world of his diseased feelings and emotions. The series problem is not so easily touched by this difficulty, since it is relatively non-contentual; the objective context is pretty well thinned out and distantly situated. To use Stransky's term, dereistic thinking and the number series are quite compatible. Numbers do not so easily jar the sensitive complexes, for they are rather remote from things and events in the social realm. To complete these progressions is something like moving in a vacuum, and this is just the sort of activity that suits the schizophrenic constitution. Recall how marvelously such a grave case of schizophrenia as Scriabine could handle the most complex mathematical and dynamic aspects of musical structure, his perfect consistency in the manipulation of *formal* problems. The paretic, although he suffers from a greater degree of *intellectual* deterioration than does the schizophrenic, is not so far removed

[2] *Op. cit.*, 458.
[3] *Ibid.*, 462.
[4] *Ibid.*
[5] *Ibid.*, 458.
[6] E. Bleuler, *Textbook of Psychiatry*, 1924, 247.

from objectivity, comparatively speaking. Although his powers of abstraction are in such a process of diminution that number has ceased to have much if any meaning to him, what little mentality he has left is not so completely out of contact with the social as is the case with the schizophrenic. The paretic may retain at least the vestiges of humor: the schizophrenic is outstandingly incapable of the humor reaction.

When *Ss* are instructed to "see whether you can find anything foolish, anything contradictory, or anything ridiculous" in a story read to them, we must recollect that the schizophrenic thought content and behavior are foolish, contradictory, and ridiculous. They are fragmentary. Hunt recognizes this when he states that "the schizophrenic considers punctuation, paragraphing, and sometimes the use of a word, but almost never does he comment upon the logic of the narrative."[7] He adds, "When instructed to read aloud, the schizophrenic usually complies without selective emphasis and without a change of facial expression. When asked subsequently what he thinks of the story, his answers usually concern only his *affective* reaction to the whole 'outing' situation described".[8]

The fourth stage in the instructions on the narrative is equivalent to a prod. The schizophrenic being typically incapable of discussion due to the inhibitory action of his disturbed affective mechanisms, there being no rapport between himself and the 'outside,' the fourth stage is absolutely necessary as a spur to wake *S* up to some sense of reality. The gist of the disorder being its absurdity, that is to say, its inconsistency, why be surprised to observe that these patients fail to perceive absurdities in a *contentual* situation possessing an *outward* reference? This order of individual has a natural resistance to perceiving things in the material world as they are. All objective events are perceived as they appear after having been scrambled in the *two*-dimensional affective mixing bowl of the schizophrenic. There is a set against finding discrepancies between the outer and the inner.

The fourth stage of the instructions on the narrative, then, illustrates how the schizophrenic must literally be shoved into contact with real events if we would expect him to perceive any absurdity at all. Bleuler gives a good example of this: "When asked to tell about his divorce, he at first knows nothing except: 'I am divorced.' 'Why are you divorced?' 'She ran away from me.' 'Why?' 'We did not get along.' 'Why?' 'Because of a child,' etc."[9]

We have referred to the two-dimensional affective life of the schizophrenic. By this we mean that for him there is no real 'solidity' of experience; it is as though the patient were wandering about without a goal idea upon the surface of a two-dimensional world, with no substantial 'outside' reference, with no *feeling* for continuity. This sort of individual does not get along with the tangible *Gestalten:* he blinds himself to them. The recognition of absurdities in the material sphere is alien to his emotionally disjointed make-up.

Hunt is in effect tautologous when he states that "the *Ss* who solved prior to Step 4 would therefore succeed only when (1) the story was integrated into a coherent sequence and when (2) the integrated story brought the comment 'it should be consistent,' or some surrogate for this verbal comment".[10] Quite so: we take it that the *Ss succeed only when they succeed.*

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[7] Hunt, *op. cit.*, 460.
[8] *Ibid.*, 462. (Italics ours.)
[9] Bleuler, *op. cit.*, 81.
[10] Hunt, *op. cit.*, 463.

Hunt asks, "Now what is it in instruction and government which functionally limits the schizophrenic in this problem?"[11] He sets forth three possibilities: "the defect may pertain to functional continuity and integrity, to functional range, or to governmental guidance or control."[12] But why make it a disjunctive matter? In view of what is known about the extremely complex mechanisms of mentality the likelihood is overwhelming that there is rather a tremendously intricate *interaction* of the general factors named. Above all, we must look to *emotional distortions* for the basic explanation and state this—if knowledge may justify—in terms of anatomo-psysiological pathology.

Clinton, N.Y. PAUL C. SQUIRES

THE FORTY-THIRD ANNUAL MEETING OF THE AMERICAN PSYCHOLOGICAL ASSOCIATION

The forty-third annual meeting of the American Psychological Association was held at the University of Michigan, Ann Arbor, Michigan, on Wednesday, Thursday, Friday, and Saturday, September 4, 5, 6, 7, 1935. A total of 648 persons registered as being in attendance, classified as follows: 150 Members, 312 Associates and newly-elected Associates, and 186 persons not affiliated with the Association.

At the business meeting, 6 Members were elected, 17 Associates were transferred to the status of Member, and 211 Associate Members were elected. The invitation of Dartmouth College to hold the 44th Annual Meeting in Hanover, New Hampshire, was accepted and the following dates were fixed: Wednesday through Saturday, September 2, 3, 4, 5, 1936. Charles Leonard Stone was appointed a member of the Executive Committee for 1935-1936, and Miles A. Tinker, J. P. Guilford, and the Secretary were elected as the Program Committee for 1936. Newly-elected officers are: President, Clark L. Hull; Directors, Harold E. Jones and Mark A. May; Division of Anthropology and Psychology, N. R. C., Edwin G. Boring, Leonard Carmichael, and Edward C. Tolman; S.S.R.C., Gordon W. Allport; A.A.A.S., Walter R. Miles, and Christian A. Ruckmick. Norman L. Munn was elected as a member of the Committee on Precautions in Animal Experimentation.

The Association voted to continue affiliation with the Inter-Society Color Council for the year 1935-1936, appointed Forrest Lee Dimmick, Sidney M. Newhall, and M. J. Zigler as voting delegates to the I.S.C.C. with F. L. Dimmick as Chairman, and appointed Frank A. Geldard, Clarence H. Graham, J. P. Guilford, Harry Helson, Theodore F. Karwoski, Elsie Murray, and Donald M. Purdy as additional delegates. The Association also voted to continue the Committee on Psychology of the National Advisory Council on Radio in Education as well as the Committee on Motion Pictures and Sound Recording Devices in Instruction of Psychology. The report of the latter Committee contained the information that abstracts of 35 films have been received and are beginning to appear in *Psychological Abstracts*. Progress reports were submitted by the Committee on the Experimental Study of Suggestion and the Committee on the Relations of the Association to its Publications and the Association voted to continue these two committees with the expectation that final reports would be submitted at the 1936 meeting. The Association authorized the establishment of a local section of the American Psychological Association in Washington, D.C. and Baltimore, Maryland with John E. Bentley as Chairman and Cora B. Key as Secretary.

[11] *Ibid.* [12] *Ibid.*

The Association voted to authorize the Council of Directors to appoint a committee to consider the Reorganization of the Association in Relation to Regional Branches and Regional Meetings. Affiliation of the newly established Psychometric Society with the Association was authorized. Consideration of the plight of unemployed psychologists led to the establishment of a Committee on the Social Utilization of Unemployed Psychologists with A. T. Poffenberger as Chairman. The committee was instructed to undertake, as a first step, a census of all unemployed psychologists. A Committee to Study Problems Connected with the Teaching of Psychology in High Schools and Junior Colleges was appointed consisting of Goodwin B. Watson as Chairman and Calvin P. Stone as a member. A Committee on Psychology and the Public Service was also established with L. J. O'Rourke as Chairman and Walter R. Miles and Harold E. Burtt as members.

Pressing publication problems were dealt with as follows: Action on a proposal to publish a cumulative index of *Psychological Index* was postponed until a more propitious time. The Association voted to approve the principle of a ten year cumulative index of *Psychological Abstracts* and authorized the Business Office to canvass the financial feasibility of undertaking its publication. The Association voted to discontinue the *Psychological Index* with the publication of the June 1936 number. Finally, the Association appointed the Editor of *Psychological Abstracts,* the Business Manager of the Review Publications, the Treasurer of the Association and Robert S. Woodworth as a Committee to negotiate for a subvention to support the publication of *Psychological Abstracts* for the next ten years and authorized increasing the present annual allotment per Member and Associate of $3.00 for the *Abstracts* to $4.00 beginning in January 1937 without change in dues.

Twenty-one formal sessions were scheduled and 120 papers were presented. Papers on animal behavior again predominated with four sessions devoted to this field. Judged by the nature of the formal papers, research activity is especially pronounced in physiological psychology and learning as well as in animal psychology. The measurement of attitudes and methods of multiple factor analysis are also receiving considerable attention. In spite of these trends in emphasis, it is reassuring to note continued research activity in such a broad array of topics as sensation and perception, memory, abnormal phenomena, galvanometric methodology, mental testing, child development studies, vocational and applied psychology, and aesthetics.

On Wednesday evening, a general session was held at which time Charles Spearman, as guest lecturer, discussed and demonstrated new tests of ability, General and Specific, Innate and Acquired. Immediately following this special lecture, eight research and instructional films were shown. One of the formal sessions was devoted to papers on Clinical Psychology and four Round Tables on Clinical Psychology using the panel discussion technique were held. The Section on Clinical Psychology elected Dr. Martin L. Reymert as Chairman for 1935-36 and Dr. Rose G. Anderson as member of the Executive Committee for 1935-38. By-laws adopted by the Section in December 1927 were amended and approved by the Association at the Business Meeting. A total of 6 Members and Associates was admitted to the Section. Additional Round Tables were also held dealing with the following topics: Psychometrics, Psychology and the Public Service, Motivation, Time Lag in Publication of Psychological Research, and A.P.A. Program Making.

On Friday evening, the Presidential Address, "Psychology and Life," was given by Albert T. Poffenberger. In brief, President Poffenberger upheld the thesis that

psychology is essentially an applied science—a fact that increases greatly the responsibilities imposed upon psychologists. If one accepts this point of view, then the policies of the American Psychological Association and the policies of graduate departments of psychology must necessarily be altered to the end that psychology will make increasingly important contributions to governmental and public questions and the label "psychologist" will come to have a definite meaning in terms of professional competence.

Following the Presidential Address, an illuminated parchment testimonial award for distinguished service to psychology was presented to James McKeen Cattell, *in absentia*. At the conclusion of this ceremony, the University of Michigan entertained members and guests at the Michigan League Building where a reception was held and light refreshments were served.

Dr. Walter B. Pillsbury, his colleagues, and the administrative officers of the University of Michigan were uniformly successful in making well-nigh perfect arrangements for the Forty-Third Annual Meeting. Even the weather was perfect.

University of Minnesota DONALD G. PATERSON

A NEW JOURNAL FOR THE SOCIAL SCIENCES

We have recently received the first issue of the *Journal of Social Philosophy; A Quarterly Devoted to a Philosophic Synthesis of the Social Sciences* (Vol. 1, No. 1, October, 1935). The main sanction given for this new periodical is "a need for critical analysis and integration experienced by an ever-growing number of scholars in the social disciplines." The new *Journal* "constitutes itself a clearing-house where ideas emanating from all the various social sciences can be made to meet and mingle in fruitful communication so as to iron out their discrepancies and arrive at some general working agreement as to common assumptions and explanations." The initial number contains articles on the place of economics in social philosophy (J. R. Commons), Education and social change (I. L. Kandel), A symposium on Pareto's significance for social theory (W. McDougall, C. Murchison, J. H. Tufts and F. N. House) and a department of brief book-reviews. Members of the editorial board are R. M. MacIver, C. L. Becker, J. Dickinson, P. Klapper, J. Viner, and M. J. Aronson (Managing Editor). General and social psychologists have been invited to contribute to the periodical. M. B.

AN INTERNATIONAL JOURNAL OF PSYCHOLOGY

Since the publication of the last number of the JOURNAL, we have received the first issue of *Acta Psychologica*. It is edited by G. Révész of Amsterdam, Holland, with the collaboration of an International Board of thirty-one members. Of this Board, four reside in America: E. G. Boring and K. S. Lashley of Harvard University, William McDougall of Duke University, and R. M. Yerkes of Yale University.

The purpose of *Acta Psychologica* is "to bring a closer contact between the different currents of thought and research in psychology." The editors of the new journal hope to realize this "by publishing contributions in monograph form written from a theoretical point of view." Contributions will be published in the English, French, and German languages, with summaries at the end of every one in the alternative languages.

The first number contains 23 papers that were read at the Tenth International Congress for Psychology at Copenhagen in September, 1932. K. M. D.

Joseph Peterson: 1878-1935

Joseph Peterson, Professor of Psychology in the George Peabody College for Teachers at Nashville, Tennessee, died in Berkeley, California, on September 20, 1935. He was born in Huntsville, Utah, on September 8, 1878. His father, Hans Jordon Peterson, and his mother, Inger Mary Christensen Peterson, came from Denmark. Dr. Peterson's earliest education was in an ungraded school which he attended two or three months each year when he could be spared from his father's farm. Later he attended the Brigham Young Academy and the Universities of Utah, California, and Chicago. He received the B.S. from Chicago in 1905 and the Ph.D. in 1907. He was a fellow at Chicago from 1905 to 1907.

From 1899 to 1904 Dr. Peterson occupied positions as school principal in Utah and Idaho. His first appointment was at Kanab, Utah. After receiving his doctorate he became Professor of Psychology in Brigham Young University, and from there he moved, in 1911, to a similar position at the University of Utah. In 1915 he left Utah to become professorial lecturer, and later he was made assistant professor at the University of Minnesota. In 1918 he served as chairman of the department of psychology at Minnesota. The call to his position as professor of psychology at the George Peabody College for Teachers came in 1918. In the summer of 1907 Dr. Peterson taught at Drake University, in the summers of 1914 and 1926 at the University of Chicago, and in the summer of 1931 at the University of California.

Dr. Peterson was an active and prominent member of several learned societies and scientific organizations. He was President of the American Psychological Association in 1934 and member of the Council of Directors of that body from 1927 to 1929. He was a member of the Division of Anthropology and Psychology of the National Research Council from 1926 to 1929 and again from 1932 until his death, at which time he was serving on the Board of the National Research Council Fellowships in the Biological Sciences. In 1931-32 he was on the Southern Regional Committee of the Social Science Research Council. He was also a member of the Society of Experimental Psychologists, the Academy of Political and Social Science, the Tennessee Academy, and fellow of the American Association for the Advancement of Science. Since coming to Peabody in 1918, Dr. Peterson had a keen interest in psychology and education in the South, and he took an active part in several surveys of public school and college teaching. In 1921 he was elected president of the Southern Society for Philosophy and Psychology. Shortly after Dr. Peterson's death President Payne of Peabody, in a letter to President Angell of Yale, voiced an estimate of Joseph Peterson's position: "You recommended him to me eighteen years ago. In doing so, you conferred the greatest kindness upon higher learning in the South that any man has ever conferred in my lifetime."

In addition to his intense labors in teaching and investigation, Dr. Peterson found time to contribute substantially as an editor of psychological publications. He was an associate editor of the *Mental Measurement Monographs* and a coöperating editor of this JOURNAL. For many years he contributed to the JOURNAL reports of the scientific papers delivered at the annual meeting of the Southern Society for Philosophy and Psychology. From 1928 he conducted the book review department of the JOURNAL and supplied a large number of reviews from his own pen. In 1934 he was elected to the arduous and exacting post of editor of the *Psychological Monographs,* a position which he occupied at the time of his death.

Although Dr. Peterson wrote from time to time upon general problems of education, he was essentially a psychologist's psychologist. He came to the subject with excellent talents for mathematics and music and his first original contribution, his doctoral dissertation, was a highly technical monograph on combination tones and other related auditory phenomenon. There was a strong interest in auditory phenomena at the time Peterson was a graduate student at Chicago and he plunged into one of the most difficult problems in the field. As we follow his productive record from 1908 on, we find that there were few major topics of psychological inquiry in which he did not take an active interest. His experimental and theoretical contributions covered a wide range: combination tones and binaural beats; musical harmony; selective factors in learning; intelligence tests; handling men in armies; transfer of training; rational learning; comparative intelligence of Negroes and Whites; summation of stimuli in learning; history of mental tests; illusion and local sign. It is significant that, of the two pieces of work in preparation at his death, one was an experimental study of inverted vision and the other a critical treatment of the intelligence of different races. Throughout a professional career of nearly thirty years, he had an unusual capacity for entering into the new developments in psychology without losing touch with the more traditional chapters.

Psychology is one of those sciences in which new knowledge comes forward painfully and slowly. Brilliant forward thrusts are striking exceptions. So far as we can judge at present, Joseph Peterson was not one of those to make a revolutionary 'discovery.' His experiments and those of his students were always clearly conceived and well-executed. His theoretical discussions always did something toward clearing the psychological atmosphere. One has the feeling that Peterson was well content to be in his shirt sleeves down among his fellow investigators and that he was never worried because his work was unspectacular. In his eyes the advancement of psychology was the thing and he conceived it as a dignified and important task to work at any of its problems whether they were novel and of general public interest, or whether they were such as are recognized only within the profession. Not that Peterson's contributions were dull or trivial. His rational learning test and many of his experiments exemplified real ingenuity. His book, *Early Conceptions and Tests of Intelligence* (1926), showed a fine sense of historical perspective. His articles on statistical questions in psychology displayed a talent for quantitative thinking. Furthermore, there ran throughout his work a sustained faith that psychology is a vital human enterprise.

The life of Joseph Peterson was a splendid realization of the ideals of nineteenth century America. His immigrant father was a farmer and mechanic who also did some carpentry and wood-turning. Joseph's early life was hard. He got up at four o'clock in order to do his lessons before time for the chores. When he was old enough for high school his father took him, together with a brother and sister and a stock of provisions, by wagon ninety miles to the academy at Provo. Joseph, his brother, and sister kept house, occasionally receiving supplies from home. Their poverty was so severe that Joseph could not afford the twenty-five cents to join his class. Nevertheless, he completed the three-year course in two and made an outstanding record, especially in mathematics. His reason for choosing psychology rather than mathematics as his life work was that the former gives more human contacts.

When Joseph Peterson first taught school in a small town in Southern Utah,

the teachers under him and even some of the students were older than he. Moreover, the man whom he succeeded had been unable to maintain discipline, but Joseph, after some interesting experiences, solved the problem. One of his duties was to direct the singing, which, after a period of timidity, he came to enjoy. Later in Idaho he directed a church choir and it was there that he met his future wife, Rhoda Richards Robinson, who played the pipe organ. Peterson always loved music and took an active part in it during the greater part of his life. For a short time he played the base viol in the Peabody College orchestra and for several years he sang in the chorus. For some years he, his wife and his sons had a family orchestra consisting of piano, first and second violins, clarinet (later changed to flute) and base viol. One of his favorite recreations was to play chess on Sunday afternoons while listening to the broadcast of the New York Philharmonic.

Peterson's home and family played a large part in his life. He walked, played tennis and studied with his sons. He was, himself, a proficient tennis player and one of his sons gained a considerable local reputation at the game. He went straight through the calculus with a son who was studying the subject in college. He also read French and German with two of the boys, and Italian which he had not studied earlier in life. It is interesting that one of his sons became a mathematician, one a musician, and the youngest a chemist.

Joseph Peterson lived the strenuous life. As his capacity matured over the years he become increasingly productive. His publications (not counting book reviews) numbered about twenty during the first decade of his professional career, forty-four during the next decade, and more than forty in the eight years prior to his death. Except for the summer of 1917, when he had the whooping cough, he taught in every summer from 1907 to 1935. In recent years he felt increasingly the need for leisure in which to carry out his many contemplated scientific projects. He often wished that he might escape his duties in the summer school, but he was held to this task by a great satisfaction in what he was accomplishing for psychology and psychologists of the South.

To his colleagues throughout the land, Joseph Peterson represented the best that a whole-hearted devotion to psychology can mean. To his students, he was "a man of quiet mien, a scholar of the highest rank, and a beloved figure on the Peabody campus."

Yale University

EDWARD S. ROBINSON
FLORENCE R. ROBINSON

EDITORIAL NOTE

The slight change in form of the cover page of this issue is made at the firm request of the associates for the past ten years of Professor Dallenbach, who has borne and who must continue to bear, the main burden of editing and publishing the JOURNAL.

MARGARET F. WASHBURN
MADISON BENTLEY
EDWIN G. BORING

BOOK REVIEWS

Edited by JOSEPH PETERSON,* Peabody College

Psychology. By ROBERT S. WOODWORTH. Third ed. New York, Henry Holt & Co., 1934. Pp. xiv, 546.

The author of a satisfactory *general* introduction to psychology is faced with a task of tremendous dimensions. He must present the fundamentals of a subject in which there is still disagreement over what, for the beginning student, the fundamentals are. He must organize the material so that the student is introduced to the field as a single and unified one, but with a minimum of involved and difficult theorizing. If the book is to be used widely, even the implicit theory behind the organization cannot cut too sharply across the theoretical biases of colleagues. He must, withal, take account of recent work in a rapidly moving set of research problems and must write lucidly and interestingly without sacrificing exactness. These criteria, to extend the sampling no further, are enough to assure textbook writers sleepless nights. That Professor Woodworth's text has met most of these or of other criteria to the satisfaction of many psychologists is guaranteed by the wide use of two preceding editions.

The outline of the third edition departs considerably from that of the second, and the content has been extensively worked over and revised. After two introductory chapters on aim and methods, come chapters on Individual Differences and Intelligence. Personality, which in the second edition was left until the last chapter, now follows the one on Intelligence and is supplemented by an additional chapter on Factors in Personality. Then come Heredity and Environment and The Development of the Individual Through Maturation and Through Activity, followed by the chapter on the Nervous System, which was next to the last in the preceding edition. Next in order are Learning, Memory, Motivation, Feeling and Emotion, The Sense of Sight: Visual Sensation and Perception, The Other Senses, Observation (including attention and perception), Thinking, and Imagination. The new order of topics is an interesting experiment and is an improvement over that of the second edition. The topics are not in logical sequence, but it may be that they can be taught better in this order.

The point of view about what the fundamentals are and the choice of specific factual material are continuous with those of the preceding edition. That most of the material presented belongs in the introductory course will be conceded by a majority of psychologists. There is a catholic choice of experimental data, "the ideal of the book, indeed, might be described as panscholastic" (p. 521). Controversial systematic issues are avoided in this panscholasticism, but there runs through it the constant thread of Woodworthian dynamic psychology, presented without argument and often implicitly. There is a functional system here; it is unnamed and loosely knit, but its outlines are clear.

* The copy for this issue of the JOURNAL was edited by Professor Peterson shortly before his death. An account of his life and an appreciation of his work appears in this issue.

The original S-R formula became S-O-R in the second edition and is now W-S-O-R-W, which means that "stimuli from the environment arouse the individual to make responsive movements which change the environment" (p. 8). The distinctive general ideas of the book are "the idea of the synthesis and interaction of parts within a pre-existing whole, and the idea that the fundamental trend of the individual is toward meeting his own needs by participation in the activities of the environment" (p. 521).

As in the preceding edition, many of the basic concepts are undefined or are defined loosely. When, for example, psychology is defined as "the science of the activities of the individual" (p. 3), how shall one differentiate between it and other biological sciences? Some of the concepts, however, are operationally defined and this is probably the optimal mode of definition for an experimentalist. The operational mode is not, however, consistently followed. The insistence that psychology's terms "are properly verbs and adverbs" (p. 29) is excellent and is needed.

Most of the special topics are given a well-balanced treatment and in practically all the influence of recent experimental work appears. On the factual side the book has moved with research. The references at the end of each chapter are to representative work, specifically cited in the text, and are not merely the conventional lists of uncited general textbooks. At the end there is a list of essay questions on each chapter.

The book is written in Professor Woodworth's easy conversational style which students read with pleasure, but which is so interesting that they sometimes forget to study what it says. It pictures psychology pretty much as it is, still diverse and unsystematized; its point of view is that of a large number of psychologists; it gains in fluidity and catholicity where it loses in closely-knit system, and it strengthens its place as a representative textbook.

University of Missouri JOHN A. MCGEOCH

The Psychology of Wants, Interests and Attitudes. By E. L. THORNDIKE et al. New York, Appleton-Century Co., 1935. Pp. x, 301.

The book deals with the dynamic effect of wants, interests and attitudes upon the process of learning. It includes descriptions of numerous experiments planned by the senior author and carried out by his collaborators. The work is divided into two parts, the first dealing with the way that wants, interests and attitudes operate in influencing behavior, and the second part discussing how they are strengthened, weakened or shifted in their attachments.

After laying down several facts which tend to establish the forward action of wants, the author proceeds to discuss various experiments which indicate the potency of interests and motives in causing the after-effects of certain connections to be satisfying. It is not that the satisfier has a retro-active effect on the connection, but rather that it causes the animal to do something which strengthens the connection to which the satisfier is attached. Experiments indicate that the satisfier does not even have to be relevant. The author recurs to his earlier work *Fundamentals of Learning* on the point that punishment tends not so much to weaken a connection as to lead the individual to do something else which may then perhaps be strengthened by satisfaction. He cites a more recent experiment where punishment even produced a slight facilitation and amplifies his point that reward is on the whole more effective than punishment as a mechanism for learning. Some rather ingenious experiments

are cited on the comparative ease of learning useful and useless material such as false dates or typing words backwards. The differences were slight but the subjects were motivated by monetary rewards or a possibility of making a good reputation as a learner in the eye of the experimenter. In other words, the extrinsic interests seemed about as effective as the intrinsic. Quite a bit of biographical and historical material is gathered and analyzed in an effort to discover instances where punishment and reward were effective in producing some type of behavior. The results are somewhat equivocal, but certainly not in favor of the efficacy of punishment.

The chapter on practical applications at the end of Part I is rather thought-provoking for the educator, just as the earlier ones will be stimulating to the reader who is interested in theories of learning. The author stresses the fact that punishment is less dependable than reward and unless it induces the person to shift immediately to the right connection which is thereupon rewarded, the punishment itself is often a waste. He criticizes the tolerance of imperfect learning in the early stages, trusting to a gradual elimination of errors, on the ground that what removes the errors is rewarding right connections, and holds that rewarding right connections might be started earlier. In certain fields the teaching consists almost entirely of pointing out errors which, according to the present hypothesis, is entirely wrong. He points out that, in teaching children, all too frequently the punishment reflects the annoyance of the punisher rather than any scientific attempt to improve the behavior of the punished.

The reviewer experiences the same difficulty that perhaps the majority of the readers will have in tearing down this familiar idol, but Thorndike does present a rather imposing array of experimental evidence. It may be that further experiments under school rather than laboratory conditions will throw additional light on some of these problems.

The second part of the book brings out the fact that wants, interests, and attitudes become modified by repetition and reward in the same way that ordinary acts become modified. The experimental material is largely in the aesthetic field, such as preferences for short verses, picture postcards, pictures of buildings, and the like. The experimental results with mere contiguity, that is ,making an aesthetic judgment for an item that is surrounded by pleasant or unpleasant items, are on the whole negative, unless some further suggestion is given to the effect that this kind of judgment is to be esteemed. On the other hand, rewarding the expression of an attitude by having the subject agree with the judgment of experts seems to be a potent factor in modifying attitudes. The importance of conditioning in this respect is somewhat minimized and the author sets over against Watson's classical experiments on conditioning the infant's fears other studies in which the attitudes are much more slowly acquired. He does not deny the mechanism altogether but tends to minimize it. Howver, he wisely suggests that if we are going to use the principle it is more valuable to use it in the positive than in the negative fashion. For instance, it would be better to condition courage or some other desirable attitude in the babies towards the situations which they might have to meet later rather than condition fear as was done in the experiment. The effects of repeating the stimuli from the standpoint of adaptation are somewhat equivocal. In some experiments there was a definite tendency for pleasant or unpleasant stimuli to regress toward indifference, but other non-experimental cases are cited in which no adaptation

appeared to occur. The upshot of this section is that wants, interests and attitudes can be taught just as surely, though not as easily, as facts or skills, and there is some hint as to the educational implications and the responsibilities of the educational system.

The appendices occupy about 70 pages and are devoted largely to descriptions of the experiments cited in the text. They also include some of the more detailed data which do not appear earlier.

The book is well written in straightforward, interesting style, and with a commendable sense of humor. While it is, perforce, technical in many places, these portions are interspersed with rather homely, interesting examples which make the point very nicely. Persons in the experimental and testing fields will find many good ideas in the appendix regarding materials to use in various types of experiments and tests. The book would also provide a good "work-out" for graduate students in evaluating critically a lot of data that are easily accessible.

Ohio State University HAROLD E. BURTT

Beauty and Human Nature. By A. R. CHANDLER. New York, Appleton-Century Co., 1934. Pp. viii, 381.

A philosopher writes a treatise on psychological aesthetics—and makes a good job of it. Here is a book that lives and will probably continue to live for many years to come. Its appeal is universal, and will grip the interest and attention of both layman and professional workers. And it serves excellently as student text. There is not a single chapter that can be counted dull. Whether we agree or disagree with the author's conclusions, we are forced to acknowledge that every page breathes meaning; and that, in keeping with the motive of the book, the author's style is a work of art, a masterpiece of simplicity, clarity, and beauty. It is safe to say that Professor Chandler's book represents the best English work in psychological aesthetics that has appeared within the last decade.

The aim of the book is primarily psychological. A serious attempt is made to fashion conclusions from experimental data which might lead to a better understanding of beauty and to an appreciation of the relations between beauty and various types of natural phenomena. The reader is presented with an excellent survey of actual results of visual form and color on the observer; of the organization of form and color in architecture, sculpture, and paintings; of the effects of tones and their organization in music; of the rhythms of speech and of the organization of sounds and meanings in literature. Yet the author warns us that his treatise of the aesthetic necessarily begins in philosophy and ends in philosophy. But this urge to begin and end in philosophy is expressed so gently and politely that it need not disturb even the hardened experimentalist.

Chandler's standpoint is essentially eclectic. The logic into which the aesthetic subject matter resolves may be either the relative or the formal variety. He selects from each of several psychological systems those methods and concepts which seem best adapted to his purposes of description and exposition. Thus we find application of the subjective as well as the objective methods of evaluating. And in the light of this eclectic position we are not unduly surprised to find some reference to intuition and the unconscious as well as to neural and muscular behavior. Fortunately for the reader with an empirical bent, the largest portion of the volume is devoted to an objective consideration and evaluation of experimental studies.

The aesthetic experience is defined as satisfaction in contemplation, or as a satisfying intuition. 'Intuition' is taken as a general term for perception, memory, and imagination. 'Satisfaction' is taken to mean a state of mind in which there is manifested a willingness to prolong or to repeat the experience in question. A typical satisfying intuition has certain definite traits; it is isolated, unified, and usually significant. Moreover, it stands in contrast with bodily, practical, and intellectual satisfaction. Whenever we view incidents of life and traits of human beings impartially, restricting our judgment to the function of interest, we have aesthetic experience. "The man who can see a joke on himself is more aesthetic than the man who wears an orchid in his buttonhole."

The varieties of aesthetic experience are in a sense relative to the aesthetic categories. In some treatises, the term *'beauty'* is used so broadly that it covers the entire field of aesthetic excellence; however, beauty is best recognized as one of several coordinate terms for aesthetic excellence. The first category in aesthetic experience is the formal factor which makes little or no appeal to any impulse. It is identified with sensory-perceptual beauty. The second category is formal beauty in its relation to sexual charm. Formal beauty, as enriched by appeal to our protective impulses, is described as the third category. The sublime, the tragic, the comic, and the interesting, are the remaining categories. Beauty and interest have their positive counterparts in their opposites ugliness and dullness, but the sublime, the tragic, and the comic have no positive antitheses.

This speculation concerning the categories and their positive counterparts gives one a suspicion that a decadent absolute idealism has been quietly spun into the mesh of an empirical frame. If such idealism is current in the initial discussion, it only serves to augment the loopholes through which the empiricist can push his own interpretations. Indeed, the description of the categories is so subtly integrated with empirical situations that both idealist and empiricist will feel a sense of satisfaction in the conclusions drawn.

The middle bulk of the volume is devoted to a description of the empirical psychology of pictorial art, architecture, sculpture, music, language, and literature. Practically a third of the book is devoted to the psychology of music and rhythms of speech. The section on music is so well developed and clearly presented that one need have little knowledge of musical technique in order to understand what it is all about. One regrets, however, that so little space under musical structure is devoted to the operatic form, especially as it is manifested through the genius of Wagner. In the section on talent and genius the author briefly reviews Kretschmer's theory of genius and abnormality and concludes that "until we have an expert study of an impartial list (here referring to Kretschmer's list of psychopathic geniuses) we should avoid dogmatic conclusions as to the relations of genius, psychopathy, and insanity." Heredity, environment, and tests of ability are considered and the avoidance of extreme views is favored. Finally, the various art tests and measures of musical talent are reviewed and critically evaluated. Chandler takes an empirical stand and holds that the development of the artist is not a mysterious thing that is beyond scientific observation and analysis. We are assured that trial and error constitute the indispensable method in all fields of learning, and that singers, composers, painters, and writers all experiment and do a great deal of bungling.

But the satisfaction which the empiricist might find in such an account of artistic development through trial and error learning must soon be dampened by the tangle of inconsistency which the eclectic position inevitably weaves. Trial and error are

not the whole story. We have to reckon with 'creative imagination' and 'psychological insight.' The creative imagination of the artist is not a substitute for information, but a way of dealing with information. Authors and composers do not use the categories of technical psychologists; they do not talk of conditioned reflexes. The psychological insight of the author is "a creature of the well." Inspiration is in a measure explicable in terms of the process by means of which unconsciously organized imagery emerges into consciousness. Shades of Freud and of the unconscious cellar! Coleridge's account of his dream poem "Kubla Khan" is offered as illustrative support for the process "by which concretions are unconsciously formed 'in the well'" and ultimately presented to consciousness ready made. Quotations from Prince are offered as additional support.

The book closes with a consideration of the nature of culture. Culture is described as a balanced and appropriate development of certain human capacities. No attempt is made to state just what capacities constitute a correct list, but the author is certain that the capacity for aesthetic appreciation should be included in whatever form the list might asume. The cultured person is responsive to the aesthetic aspect of nature and the arts, and his responsiveness is characterized by a kind of median enthusiasm. In the really cultured person we find an equilibrium maintained by a moderate variety of interest, which favors the growth of appreciation and discrimination. Culture in the aesthetic sense consists in a well-balanced development of our powers of appreciation. It is through a combination of interest and discrimination that we acquire appreciation—the recognition of values and the enjoyment of them. Appreciation may be cultivated by contact with works of art serially graded according to the best known methods, by practice and active participations in the arts, and by direct or indirect association with persons who appreciate the arts.

After some consideration of the nature of criticism and the rôle of artists as critics, a closing section is devoted to a discussion of the relations between aesthetic values and those of science, religion, and business. A list of selected references is appended to each chapter.

Florida State College for Women CHRISTIAN PAUL HEINLEIN

Comparative Psychology. Edited by F. A. Moss. New York, Prentice-Hall, 1934. Pp. xiii, 529.

In this excellent and well unified volume the editor has brought together a series of separate chapters, each written by a thoroughly competent specialist.

In the first chapter E. L. Thorndike clearly answers the question, "Why Study Animal Psychology?" by pointing to the significance of this branch of study in any complete understanding of mental life. "The Historical Background of Comparative Psychology," by R. H. Waters, continues the introduction by showing the evolution of attitudes and techniques in comparative psychology.

The next two chapters, "Maturation and 'Instinctive' Functions" and "Motivation: Incentives and Drives," both by C. P. Stone, admirably summarize an involved and rapidly growing literature. These two chapters also present what seems to the reviewer to be the major theoretical problems of these involved fields in a clear and valuable way. Possibly the author's point of view may best be summarized by saying that to him maturational development is always primary and development by learning secondary.

In a chapter entitled "The Effects of Drugs and Internal Secretions on Animal

Behavior," F. A. Moss, the editor of the volume, has himself written an excellent summary of much material that has never before been generally available to students of animal psychology. The description of infra-human drug addiction given here is especially noteworthy.

D. M. Purdy's chapter on "The Functions of the Receptors" is a model of condensation and clarity. The material that is reviewed and organized in this chapter is modern and important. The reviewer wishes that Dr. Purdy had been allowed more space for the discussion of the significant problems with which he deals.

The chapter on "Discrimination" by P. E. Fields must, by force of its subject matter, deal with results that are still highly controversial. The author of this section has, however, clearly presented one point of view in regard to this subject.

S. I. Franz's chapter, "The Neurology of Learning," written as it was shortly before his death, is certain to remain one of the important contributions of the volume. It presents in clear and simple form this eminent student's sane and thoughtful view of the essential neurology of learning. It is a scientific and factual account of certain aspects of a field in which there is even today much speculation and much alleged "explanation" by analogy.

The next four chapters of the book are devoted to separate aspects of the problem of learning. H. S. Liddell writes on "The Conditioned Reflex," W. T. Heron on "Learning" and "Complex Learning Processes," and E. C. Tolman on "Theories of Learning." In spite of the different backgrounds and theoretical positions of these writers, the student who reads these chapters cannot fail to be impressed by the basic scientific nature of the work that has been done and is being done in this central psychological field. Surely the space devoted to these topics is well spent, for to the student of comparative psychology no subject can be more significant than learning. Moreover, as Thorndike points out in the introductory chapter of this very volume, the facts discovered by studying animal behavior in learning situations provide "general principles that may be adequate to account for much, or even all, of human learning."

The three remaining chapters of the book, "Individual Differences," by R. C. Tryon, "Social Psychology of Animals," and "'Gifted' Animals," by O. L. Tinklepaugh, give in convenient form a summary and interpretation of work in animal behavior which has never in the reviewer's opinion been so adequately organized before.

All in all, therefore, this volume seems not only to be a successful student manual, but also to be in many respects a real contribution to the professional literature of psychology.

Brown University LEONARD CARMICHAEL

Mental Defect. By LIONEL S. PENROSE. New York, Farrar & Rinehart, 1934. Pp. xii, 205.

"This book is intended for the use of medical or lay persons who desire to obtain information on some of the scientific problems associated with the study of mental deficiency." Nevertheless, it is fairly certain that American medical men and any others who may chance to read it will get more irritation than instruction from its pages. To begin with, it has no index! (If authors perpetrate such discourtesy to their readers, why do publishers allow it?) In six of the sixteen chapters "the methods of investigation applicable to this branch of study are set forth" (Preface).

The author is not clear in his concepts and consequently there is much confusion and contradiction throughout the book. For example in the preface he says: "I consider the study of mental deficiency to be a branch of human biology." On page 13: "Thus mental defect is fundamentally a social, and not simply a biological or psychological problem." He later (p. 60) contradicts this: "Mental deficiency is a group of diseases and symptoms which require investigation and treatment in exactly the same way as other diseases." This is the fundamental error that runs through this book and leads to endless confusion. Why keep up the fiction that feeble-mindedness is a disease, when the great mass of the feeble-minded, given the most thorough examination that medical science is capable of, would be—and are continually being —pronounced healthy and free from disease. The author himself objects to certain treatments for the feeble-minded, on the ground that it is often impossible to tell who is feeble-minded and who is not. A strange disease surely, which has no symptoms, no lesions, cannot be cured or the condition modified by treatment; and yet the patient is comfortable, happy and as long-lived as the rest of us in many cases! He may become diseased; but then he has the same kind of diseases that the rest of us have, and treatment has the same effect.

Chapter V, on the psychological examination of the mental defective leaves much to be desired. Strange errors occur repeatedly. We read (p. 47): "The Binet scale, like many others, is composed of a number of different types of problems. The total score of the subject represents an arithmetical sum of the marks obtained on the various constituent tests. This method of scoring is quite arbitrary and has no sound theoretical basis." Has the author never seen a Binet test? He is talking about a "point scale." The Binet is, of course, not a point scale—except the little-used Herring revision. Penrose further says: "By the introduction of performance tests, with such apparatus as form-boards and blocks, it was hoped to remedy some of these limitations." A bad guess! The form-board is a half century older than any Binet test. Moreover the performance tests, as everybody else knows, were *not* introduced to "remedy some of these limitations." They were introduced primarily to obviate the difficulties of language; secondarily it was easier to get *en rapport* with small children by giving them something to do than by talking with them. Later it was found that the performance tests had other values—with which our author is apparently entirely unfamiliar.

The later chapters are somewhat more valuable, dealing as they do with medical matters where this author is more at home. The American reader will be a little annoyed at the confusion of feeble-mindedness with insanity. But that is the English point of view. Mental defect suggests both conditions just as with us in America "abnormal" still suggests the gifted child as well as the defective child.

Dr. Penrose has evidently read fairly widely, but perhaps too rapidly. The book gives the impression of having been hastily written, and on the basis of too little experience.

Ohio State University HENRY H. GODDARD

Psychology and Health. By H. BANISTER. New York, Macmillan Co., 1935. Pp. viii, 256.

Dr. Banister has found, as have many others, that "the skilled physician must be a psychologist with a knowledge of the workings of that most intricate of things, the human mind." But the addition of adequate psychological content to the already

heavy medical curriculum is a hope for the future. For the present, a manual like this book will be extremely useful in providing an outline of modern psychopathology that is of value to the medical practitioner who is not a specialist in nervous and mental disorders.

The average physician finds himself confronted with behavioral difficulties in children and adults that do not respond to the ordinary medical therapy. Yet, in his capacity of physician, the practitioner should be prepared to make some efforts to the end of correction. Such efforts must involve psycho-therapy, which to be successful must be based on an understanding of the psycho-etiologic factors underlying the behavior. Thus, in children who are not mentally deficient behavior problems appear as the ill-effects of the environment, more often that of the home. Treatment in such cases must be directed to changing the environment and the child's attitude toward it.

In the adult, behavioral difficulties are exhibited in the form of psychoneuroses. (Parenthetically, we may point out that the psychoses are not discussed because the author feels they are the concern of the specialist.) The close connection between the normal adjustment to difficult situations in life and the psychoneurotic adjustment is interestingly developed. That humans may act without being aware of reasons for their acts is illustrated by hypnotism, which in turn is shown to be "hetero-suggestion at its strongest." Conversion hysteria, anxiety states, compulsions and obsessions all exhibit behavior symptomatic of conflicts in adjustment of which the individual is not aware. Therapy in these conditions involves suggestion and analysis. Hysteria and some anxiety states may be successfully treated by suggestion and hypnosis. In severe anxiety states and psychasthenias resort must be had to analysis. The author points out that while analysis may explore the unconscious it is not limited to such exploration.

The author's theoretical position might be called an "eclectic dynamic psychiatry." The theories of all modern schools of psychopathology are used to illustrate and support the discussion. In four chapters he gives brief and clear accounts of the theories of Janet, Freud, Jung, and Adler. In the concluding chapter, the author presents his own theory of a "sentiment-forming tendency" which he feels is important in character development. According to this theory there is an innate tendency to form sentiments which unfolds during childhood in three stages. First, sentiments are formed about inanimate objects, then around animate as well as inanimate objects, and finally, around abstract concepts. Individual differences in sentiment formation are the result of individual factors such as intelligence and temperament, and of the differences in environments.

This book was not written for psychologists, but may prove of interest to them. It should be called to the attention of medical colleagues and medical students, and might well serve as a text in a short course in mental hygiene.

Indiana University C. M. LOUTTIT

The Physical and Mental Growth of Prematurely Born Children. By JULIUS H. HESS, GEORGE J. MOHR and PHYLLIS F. BARTELME. Chicago, Univ. Chicago Press, 1934. Pp. xxiii, 449.

Having had occasion a year ago to search out literature on the mental development of the prematurely born child, I was amazed at the paucity of studies. When I began to search for cases to study from the Indiana University Hospitals and the

Indiana State Board of Health, difficulties were soon evident. It appeared that the only place in which a study on these children could be made would be in a hospital where special facilities were available. The present study was made under just such conditions.

Part II of this monograph on the development of the premature child is written by Drs. Mohr and Bartelme. These investigators studied a group of 250 premature children all of whom had been patients in the Premature Infant Station at Sarah Morris Hospital of Michael Reese Hospital, Chicago. The physical and mental growth of these children was measured over periods from about three to eight years. In weight and stature the premature children are below their full term siblings, and the height-weight norms, until about three or four years of age; afterward there is no significant difference. Mental development was measured with Gesell's Developmental Schedule and the Binet scale. During the first two years the scores of prematures on the Gesell schedule indicate some retardation if the actual C.A. is used. If the C.A. is corrected for the amount of prematurity there is no significant difference between the premature children and their full-term siblings, or the performance of the groups used to standardize the Gesell or Binet scales. Data were also collected on habit formation (sitting-up, walking, speech, etc.) and social adaptation. Here again there appeared to be no significant differences except that the premature children were somewhat more dependent on their mothers, and exhibited personality characteristics of an "inadequacy" type more frequently. The presentation of these data is greatly clarified by a free use of tables and growth curves. There can be no doubt that this study will become a classic in the field of child psychology.

Part II comprises some 160 pages from a total of 450. The rest of the book is primarily of medical interest. In Part I, Dr. Hess describes the Premature Infant Station and presents a clinical study of the 1,922 children cared for in the Station. Part III includes ten specialized studies by as many authors reprinted from medical journals.

Indiana University C. M. LOUTTIT

Psychology and Psychotherapy. By WILLIAM BROWN. Baltimore, William Wood & Co., 1934. Pp. 252.

Psychology, the savior of civilization, is the fond dream of Dr. Brown. To Freud is given the chief, but not the sole responsibility "for bringing the science to maturity." McDougall, C. S. Myers and Spearman are given a share in the honors. The present-day ultra-nationalistic tendencies are reflected in the book by the scant notice given to French, German and American psychologists.

Dr. Brown's claim to "training and research in experimental psychology in a university laboratory" is not borne out by the subject matter of the book nor in its treatment. His psychology is a mixture of Freud, McDougall's "instincts" and his own hypnosis—and suggestion-therapy observations. Systematically it is a naïve interactionism, with the expressed belief that suggestion may produce organic changes in the person accepting it. Of modern academic psychology, behaviorism is lightly disposed of by a trick of questionable logic, and *Gestalt* is dismissed with a short paragraph. The title of the book would lead one to expect a somewhat more adequate presentation of contemporary thought in psychology.

The author's psychotherapy is hardly less objectionable. Psychiatrists would be interested to learn from Dr. Brown that the psychoses are markedly hereditary and

give no scope to psychotherapy. They would agree that the author's psychotherapeutic methods, hypnosis for the production of psycho-catharsis and abreaction and suggestion "addressed to the unconscious mind" offer little help in these cases, at least.

Thus biased, the volume is limited in its value as a text book; for advanced study it is much too naïve. There is nothing new in the book for either psychiatrists or psychologists, academic or clinical, and much that is stated in a dogmatic fashion is not consistent with advanced theory and practice. It is somewhat entertaining as a mild philosophical ramble, although interrupted by case histories, through the dark woods of suggestion, hypnotism and faith, the psychology of personal influence, the psychology of peace and war, psychical research and eternal values.

The reviewer is unable to share the optimism for the future of the psychology expressed by Dr. Brown.

Boston Psychopathic Hospital N. GOLDMAN

Training in Psychiatric Social Work. By SARAH H. SWIFT. New York, The Commonwealth Fund, 1934. Pp. xii, 177.

Professional psychologists who are responsible for the training of graduate students will be attracted by the statement in the foreword of this book that the psychologists who came to these centers for training in psychiatric social work were competent to handle mental tests, but that they "usually lacked clinical experience in the interpretation of these tests in relation to other findings, and had only rarely had experience in working with other people professionally engaged in the study of human behavior." There is also a useful sketch of the history of these schools for psychiatric social work and of the evolution that has taken place in their methods. And there are careful analyses of the student's work, sometimes expressed in tables presenting the student's case load, length of time on individual cases, and so on. The students themselves appear to be almost as carefully observed and studied as are the patients with whom the students work. The consideration of their motivations, inhibitions, conflicts and the like are considered essential features in the course of their training. The book is well written, well arranged and satisfactorily indexed. It is obviously and intentionally, however, far more of a contribution to social and educational work than it is a contribution to psychology.

Indiana University EDMUND S. CONKLIN

The Man in the Street and the New Psychology. By R. M. HOWDEN. New York, Oxford University Press, 1935. Pp. 77.

'New Psychology' is a misnomer; the book is a short and clear exposition of psychoanalysis, written "by a layman for laymen" (from the standpoint of medicine). There is no evidence that the author is acquainted with experimental psychology as it exists today in research centers. Instead of the usual "proofs" of psychoanalysis ("as everbody knows," etc.), one finds here just as worthless evidence ("statistics go to prove this contention," p. 76; "clinical evidence of innumerable patients . . . proves," p. 23; "the proof . . . is based on irrefutable evidence," p. 5; etc.) with no show of data whatever nor references to scientific experiments.

The reader is told that "Nothing we do is without a cause" (p. 60), yet "the unconscious" is assumed to 'explain' processes of which one is unaware, evidently because they cannot merely be attributed to consciousness. "The existence of the

unconscious is a scientific fact." Yes; if we mean by "the unconscious" neural and other organic processes of which the individual is not directly aware. No; if, as used by the author, it implies an unconditioned force of some mysterious sort, and every explanation based on such a fiction is invalid. All mechanisms so-called of 'projection,' 'identification,' 'displacement,' 'the pleasure-pain principle,' 'the idea of the libido as a stream,' etc. suffer from similar mystic and vague thinking. But the author admits the operation of such "other instincts" as "aggression (. . . the Will to Power), self-preservation, curiosity," etc., as well as of Adler's organic inferiority. Normality or abnormality of personality may "also be affected by:—(i) Definite predisposition to develop neurotic tendencies. (ii) Environment, upbringing, and education. (iii) The influence of endocrine glands. . . . (iv) Some shock of a severe nature in childhood, such as violently emotional sex experience" (54 f.). But little use is made of these more sensible suggestions. The Oedipus and Electora fictions are still involved as explanations despite the investigations of C. P. Stone and of J. E. Anderson, which receive no attention or citation at all. (Can a Freudian learn from experiments?) Victorian prudery and other neglects of sex education by parents are appropriately condemned.

J. P.

Dreams in Old Norse Literature and Their Affinities in Folklore, with an Appendix Containing the Icelandic Texts and Translations. By G. D. KELCHNER. Cambridge, University Press, 1935. Pp. 154.

As the title suggests, this book is a study in literature rather than psychology. It presents a piece of research undertaking to examine the contents of dreams embodied in ancient Norse (particularly Icelandic) prose and poetry and to trace their affinities in folklore. The author makes no attempt to link her research with psychological interpretation: neither the names or ideas of Freud, Jung, McDougall or other exponents of dream psychology are anywhere mentioned with the sole exception of a bibliographical reference to Rivers. Nevertheless, the student of dream psychology would find in this book some interesting examples of symbolism as these express the early Norse beliefs and their subsequent modification under Christianizing influences.

Lawrence College J. H. GRIFFITHS

The Neural Basis of Thought. By GEORGE G. CAMPION and GRAFTON E. SMITH. New York, Harcourt, Brace & Co., 1934. Pp. v, 167, 2 pl.

An attempt is made to construct a neurological basis for the theory of knowledge presented by Campion in an earlier work "Elements in Thought and Emotion." The "concept" is adopted as the mental element, and the "engram" as its neural correlate. The growth of the mental element in all its complex phases is congruous with neural processes forming complex "engrammic systems" between the neurones of different areas of the brain. The essential thalamic organs play an important rôle in activating the cortical engrams which underlie such psychological phenomena as memory and reverie. The connections between the cortex and thalamus form a neural basis for a continuous circulation between these important centers thus keeping active those "engrammic" correlates of the ideas or concepts which may be employed in any particular phase of consciousness. A strictly atomistic analysis is adhered to throughout

and no attempt is made to acknowledge or refute the more recent advances in neurology and psychology which have been away from an elemental interpretation.

University of California C. W. BROWN

Die Versicherung als Geistige Schöpfung des Wirtschaftslebens. By J. SCHREIEGG. Leipzig, Teubner, 1934. Pp. 90.

Those psychologists who may be interested in the psychological factors which are capable of contributing to socio-economic enterprises, let us say like the N.R.A. and the multitude of similar contemporaneous phenomena, ought to find this book of value through its methodological suggestiveness. Here is an institution carefully analyzed which has a long history. We learn that insurance has an intensive history of three hundred years, a radical history of more than six hundred years, and a history by analogous institutions of about two thousand years. From the historical facts stated the author derives a number of psychological principles which clearly contribute to the invention and success of various classes of insurance and, in the opinion of the reviewer, might be made to contribute to other and future economic institutions. The last thirty pages of the book are devoted to their description.

University of Missouri MAX F. MEYER

Why We Feel That Way. By AUGUSTUS W. TRETTIEN. Boston, Stratford Co., 1935. Pp. 452.

This book is a very general psychology of "emotion" written largely from a genetic point of view. Here are two examples of its treatment:

"All forms of pain are the result of a disintegration of the brain-cells. They are always associated with the motor system. The motor defense mechanism whether in teeth or claws are tied up directly within the reflex arc circuit, ready for immediate and direct action of relief. Fear, the oldest and strongest of emotions, is the product of the changed condition of the brain-cells due to a specific conditioned reflex" (p. 72).

"There is also a neural connection between the upper and lower ganglia of the autonomic system and the medulla and thus to the cerebral centers. Strong emotions of anger may take away our appetite by drawing upon our stored reserve; whereas a hungry stomach may cause a rebellion in a baby as well as in a nation . . ." (p. 87).

Institute for Juvenile Research C. W. DARROW
Chicago, Ill.

The Art of Study. By T. H. PEAR. New York, E. P. Dutton & Co., Inc., 1931. Pp. 114.

This author attacks the problem of determining the best study techniques completely from the point of view of the psychologist. He considers the art of study equal to that of the art of learning—perhaps properly. He considers what learning is, how animals learn and how we learn, differences in ease of learning, how to listen, how to concentrate, habits of study, the economy and practical aids of memorizing, what is a good memory, its possibility of training, progress in learning, and intelligence and stupidity. How to take notes, how to prepare for and take examinations, how to improve reading speed and comprehension, how to use the library, and the like are completely lacking subjects, although usually included in texts on how to study.

Within the topical limits set by the author, the book is interestingly written and should be fairly challenging to the student. The discussions of how to concentrate, to listen, and to form habits of study are especially rich in content and pleasing in style.

University of Buffalo — M. E. WAGNER

Guiding Your Child Through the Formative Years: From Birth to the Age of Five. By WINIFRED DE KOK. New York, Emerson Books, 1935. Pp. 191.

In this book Mrs. De Kok considers most of the problems which arise with young children, especially in the parent-child relations. Her interpretations are psycho-analytical and are presented in non-technical language—for the most part by means of illustrative examples from experiences with her two young children. Much of the book contains excellent advice, but there are several statements which would not be generally accepted by child psychologists, and some which are even contrary to experimental findings. She does not refer to any experimental or psychoanalytic literature, but appears to assume that her own children are typical, and so give adequate support to her statements. There is a short index but no bibliography.

University of California — NANCY BAYLEY

The Wise Choice of Toys. By ETHEL KAWIN. Chicago, University of Chicago Press, 1934. Pp. xi, 111.

This little educational monograph consists of ten lectures given by Miss Kawin in the course of a coöperative demonstration of toys arranged by a Chicago department store and the University of Chicago. The text is simple and should prove helpful to parents who have more than the average amount of money to spend. As might be expected, the possibilities of constructing durable and suitable toys in the home are not stressed.

The State University of Iowa — GEORGE D. STODDARD

Mental Health, Past, Present and Future. By A. H. RUGGLES. Baltimore, Williams & Wilkins Co., 1934. Pp. 104.

This book, representing the Colver Lectures in Brown University for 1932, is almost exclusively historical and philosophical, though a fairly accurate picture of the importance of mental disorders as a health problem is presented. Probably it has nothing meriting the attention of psychologists, unless, perchance, psychologists should wish to contrast these lectures with the recent report of Bentley, Cowdry, et al. *The Problem of Mental Disorders.*

Louisville, Kentucky — MILTON B. JENSEN

Students' Guide to Efficient Study. By L. C. PRESSEY and J. M. FERGUSON. New York, R. R. Smith, Inc., 1931. Pp. 39.

This booklet contains short chapters on planning your work, learning to concentrate, reading and studying, note taking, reviews and examinations, memorizing, developing correct general habits, written expression, previous preparation, and fitting the manual to your own needs. It is made up of 36 simple well-illustrated rules of study. These should be very stimulating and useful to high school students of all levels as well as college freshmen. It is highly recommended for student use.

The University of Buffalo — M. E. WAGNER

The Conception of Self in English Association Psychology. By INDRA SEN. Quakenbrück, C. Trute, 1933. Pp. 53.

This dissertation is primarily a compilation of and a commentary upon a series of quotations from the English Associationists. The author's conclusion is that for the Associationists the self is "the sum-total of our experiences." It is unfortunate that the publishers have permitted the author's poor English to render his thought unclear.

Bethel, Maine H. G. BISHOP

The Psycho-Biology of Language. By G. K. ZIPF. Boston, Houghton Mifflin Co., 1935. Pp. ix + 336.

This report of an extensive investigation of speech as a natural phenomenon is a first step in an attempt to bring the study of language more into line with the exact sciences. The primary aim was to observe, measure, and to formulate into tentative laws the underlying forces which impel and direct linguistic expression. Essentially the method is application of statistics to observable speech phenomena.

In general the author, by these statistical procedures, traces the effects of frequency of usage upon the form of words and phonemes, accent within the word, and the dynamics of sentence structure. Frequency of usage tends toward simplification of speech elements. This inverse relationship between frequency of usage and complexity or magnitude of language forms revealed the existence of an underlying "law of economy of effort." Unfortunately the author employed the word frequencies of Eldridge, published in 1911, rather than the more recent and more adequate Thorndike lists.

The final chapter on the stream of speech and its relation to the totality of behavior, although theoretical, is conservative for material which is controversial to a high degree. The book as a whole may be considered a desirable step toward a somewhat new and a decidedly healthy viewpoint in the study of language.

University of Minnesota MILES A. TINKER

Ghosts I Have Talked With. By HENRY C. MCCOMAS. Baltimore, Williams & Wilkins Co., 1935. Pp. vii, 192.

After an introductory chapter, which shows how and why the experimental psychologist is particularly well-equipped to investigate the 'claims' of psychical research, Dr. McComas gives a straightforward and interesting account of the studies that he made while an investigator for the American Society for Psychical Research. His results are flatly negative. Fraud, malobservation, and chance suffice to explain all the 'claims' that he examined. He is, however, sincere and disinterested in his treatment of the results, and is neither derisive or scornful of any of the 'claims' that came within his purview. The book closes with a chapter on "Why we are superstitious," which explains why, as the author states in his Foreword (p. v), "no one who believes in spiritualism ever changes his opinion by reading arguments against it." K. M. D.